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THE TRANSACTIONS

OF THE

EDINBURGH OBSTETRICAL SOCIETY.

SESSIONS 1869-70-71.

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PUBLISHERS TO THE SOCIETY.
1872.

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PREFACE.

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THE Publication Committee of the Edinburgh Obstetrical Society have now the gratification to present a Second Volume of the Society's *Transactions*, which embraces the communications brought before the Society during the Sessions 1869–70 and 1870–71.

Beyond a few verbal corrections, they have not considered it advisable to interfere with the communications, as their authors were requested to revise them before they were sent to the Committee. The discussions only embrace such portions as are relevant to the subjects brought before the Society.

The communications and discussions represent individual opinions only, and are not to be received as the expressions of opinion by the Society as a body.

On the part of the Society, the Committee cannot but refer to the great loss the Society has sustained by the death of its most distinguished ornament, Sir James Young Simpson, whose valuable contributions and eminent services to obstetric and other sciences are admitted by all to be of world-wide reputation. From that he was amongst its earliest Fellows, from the great interest he took in its progress, even up to the time of his death (his last appearance in public having been at one of our ordinary meetings), and from the numerous communications he submitted to it, the Society cannot allow this opportunity to pass without recording how deeply it has deplored his loss.

The Publication Committee congratulate the Society on the amount of creditable work submitted to it during the past two Sessions, and feel bound to express, in its name, thanks to those gentlemen who have contributed to its success.

The meetings of the Society are usually of the most practical and conversational character, admitting of the freest intercommunication of individual observation and experience, consequently, as the attending members well know, there is much valuable matter thus orally expressed unrecorded in the *Transactions*. While it is desirable that the conversational character which has hitherto distinguished the Society's meetings should be continued, it is of the greatest consequence that the verbal matter, as well as written communications, should, as far as possible, be made to appear on record; and the Committee also hope that in future the publications of the *Transactions* will redound more and more to the credit of the Society, and to the advancement of those great interests for which it is associated.

The Publication Committee again express their cordial thanks to Dr Ritchie for having a second time edited the *Transactions* of the Society.

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CHARLES BELL.
ALEXANDER KEILLER.
JAMES YOUNG.
JAMES A. SIDEY.
WILLIAM ZEIGLER.
JAMES ANDREW.
R. PEEL RITCHIE.
Members of Publication Committee.

5 St ANDREW SQUARE,
September 1872.

THE EDINBURGH OBSTETRICAL SOCIETY,

INSTITUTED 1840.

—o—

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TRANSACTIONS

OF THE

OBSTETRICAL SOCIETY OF EDINBURGH,

FOR SESSION 1869-70.

CASE OF RIGID OS TREATED BY HYDROCHLORATE OF APOMORPHIA.

By Alexander Milne, M.D.

November 10, 1869.

In June last I was called to attend Mrs S., Melville Terrace, in labour with her fourth child. On arrival, I found she had been suffering for three or four hours, and the pains were pretty strong. The os was about the size of a sixpence, and rather hard. The head was below the brim. Previous labours natural and easy. Two hours after, on making a second examination, I was rather astonished to find that, despite pains of a second stage character, little, if any, progress had been made. I introduced my finger within the os, and describing its circumference rather firmly, I found it intensely firm and resistant. I then placed the patient under the influence of chloroform, and commenced to dilate cautiously with the finger. Little good, however, was done by these means, for at the end of eight hours, the os still remained about the same size. I then procured a little tartarated antimony, giving two grains in two hours. This, though producing vomiting and ample nausea, secured neither the softening nor the expansion of the os. As the pains were continuing rather strongly, I administered 25 minims of liq. morph. hydrochlor, and left for some time. On returning three hours afterwards, I found that matters were still much the same, the morphia neither having moderated the pains nor helped to reduce

the induration of the os. Having brought with me a little hydrochlorate of apomorphia, I resolved on giving it a trial, being aware of its power as an emetic. A solution containing gr. ¼ was given. In about ten minutes the patient vomited, and the pains were subdued a little, being less frequent and forcible. On examining again, to my delight the os had softened and expanded immensely, a single sweep of the finger serving to complete it. The pains redoubled their force, and, in a few minutes more, delivery was accomplished.

REMARKS.—Cases of rigid and unyielding os are familiar to most obstetricians who have seen much practice, and there are few of us but have spent many weary hours over instances of this kind. I would, therefore, not have thought of reporting it, but for the fact that I obtained such efficient aid from a new drug, and that probably we have got a novel uterine relaxant of considerable power. I say probably, because a single case can no more test a remedy—establish a rule—than a few green leaves can make a summer. It is true that the very rigid os relaxed in a twinkling almost after the apomorphia had time to act, as if the one were cause and the other effect; but then the other relaxants may have helped to pave the way, and the growing exhaustion of the patient may also have pioneered. Further trial is therefore requisite before we can laureate it much, and this I intend giving it as opportunity offers. If it should maintain its character as a uterine relaxant—the antipodes of ergot—and effectually throw open the door of the uterus in extreme cases, I need hardly say that we shall have obtained a drug of unspeakable value; for a fundus and body contracting powerfully, and an os refusing to expand, is about one of the most distressing complications in midwifery. It is a house divided against itself, a perilous antagonism, and has been fruitful of such deplorable results as inflammation, rupture of the uterus, convulsions, exhaustion, and death. For myself, there are many other complications that I would prefer before it. The medicines that I employed

prior to the apomorphia are those commonly used, and in general prove effectual; the chloroform and antimony, with a little fingering, proving a good tripod on which to rest in these cases. Here they were ineffectual. I may state that I have long since abandoned belladonna, from a conviction that, however infallibly it may dilate the sphincter of the eye, it is next to useless here. I have never bled in such cases, believing that the remedy is worse than the disease,—that is to say, while you may secure in some cases the solution of the rigidity, you do it at a very great ransom; you enfeeble a patient already weak, and the subject of other causes and sources of debility. Incising the os is a line of treatment I have only once had occasion to adopt,—viz., in a case of cancer; and although it is supported by great names, and warranted by considerable success, yet we have always the feeling that the incisions may go further than we desiderate, and that the patient may, in consequence, fare all the worse. Of course there will always cast up, now and then, cases beyond the power of medicine, and amenable only to the scissors or knife; but wherever medicaments can accomplish the work, they ought to be employed, because less beset with danger.

I do not know that we can hope for much benefit from the apomorphia in cases of undeveloped os and cervix, such as in some cases of placenta prævia; at all events it ought to have a trial. Being also a sedative, it may in some instances lay an arrestment on the pains, the presence of which, until the os be dilated, is a source of peril. If it fulfil the twofold function of opening up the os uteri, and laying for a time an embargo on the uterine contractions, it will solve a difficult problem in this complication, and confer inestimable good.

In reference to the mode of administration of this drug, I may remark that it is much more powerful when injected into the cellular tissue. Were the stomach in any case, therefore, rejecting most things, it could be given by means of Wood's syringe.

A FIBROUS TUMOUR REMOVED FROM THE UTERUS.

EXHIBITED BY J. MATTHEWS DUNCAN, M.D.

NOVEMBER 10, 1869.

DR DUNCAN exhibited a fibrous tumour removed from the uterus by Dr James Whiteford, B.A., Greenock.

REMOVAL OF A LARGE UTERINE FIBROUS POLYPUS.

BY J. MATTHEWS DUNCAN, M.D.

NOVEMBER 10, 1869.

MRS B., from Orkney, was recommended to my care in the Royal Infirmary, by Dr Thomas Keith. She is aged 44 years; is married, and has had one child, fifteen years ago.

Twelve years ago, she first felt a lump in the lower belly. It has given her much pain. Sometimes she has attacks of very specially severe pains, which last for variable times, and are not accompanied by fever. Till last winter, her monthly periods have been regular; then they ceased; but latterly she has again been regular in this way. Ever since she felt the tumour she has had a profuse thin purulent discharge. In the early years of the disease she had profuse floodings, more frequently than in the latter; but just lately the bleedings have occasionally been very great.

1*st September* 1869.—Examined to-day. She is pale, anemic, and very sallow. In the hypogastrium is found a large prominent mass, which rises considerably above the umbilicus. There is one rounded tumour, with uniform smooth surface, whose upper border reaches the navel; and above this, and connected with it, are several smaller tumours, each about the size of a large hen's egg.

Per vaginam, a rounded free lump can be felt occupying the pelvis, as the mature fœtal head does in the beginning of the second stage of labour. The os uteri cannot be felt. On the surface of the tumour, at a distance from its lowest part, and about as high as the finger reaches, short lip-like projections can be felt. The finger pressed between these and the mass of the tumour separates them to a slight extent, lacerating what feel like loose adhesions. There is a constant very copious thin purulent discharge, and besides, during examination, there escapes an occasional small gush of viscid fluid like that from an ovarian cyst.

21*st September*.—Assisted by Dr Thomas Keith, I proceeded to attempt the removal of the large tumour. The hand introduced into the vagina was passed by the side of the tumour as high as the umbilicus. The tumour was found to be connected to the uterus by a broad large base, probably a little less than the surface of a transverse section of the tumour (or of a large fœtal head in a vertical direction). The tumour was now seized by strong volsellæ, and dragged with all the force that could be applied, but without effect; the perineum being stronger than all our efforts. We failed to pull it out. A large and strong curved écraseur was now used. The noose was passed around the tumour as high as it could be pushed, and the instrument worked. It soon gave way, and was removed. Now it only remained to remove the tumour either piece by piece, or by so cutting it, as, while removing it, to avoid completely detaching the removed part, that is, by a spiral cut. In imperfectly carrying out this last plan, the removed part was repeatedly torn off, the part remaining in the vagina. As the removal in this way progressed, the projecting œdematous lip-like parts were brought into view. They were easily identified as swollen bits of the edge of the capsule, which did not cover the tumour lower down than them. Besides now dragging and spirally cutting the tumour, the capsule was detached and pushed back. At last the

whole tumour was removed. It was torn away, and enucleated at the base. Its attachments were not cut through. A flap of investing capsule, as big and thick as the hand, was now found hanging from the uterus; it was removed by scissors. There was very little bleeding. The perineum was considerably lacerated. The proceedings occupied an hour and a-half.

There was great exhaustion after the operation. But the woman made a good recovery, uninterrupted except by a rigor on the evening of the third day, whose effects lasted for twenty-four hours. During convalescence, there was copious purulent discharge from the vagina, but it gradually lessened, and when she left town about a month after the operation, it was greatly reduced in quantity. The swelling of the belly was quite gone, and the small tumours could just be felt on pressing deeply from above into the brim of the pelvis. The tumour was an ordinary fibrous tumour; it weighed 2 lbs. 6½ oz. Its lower presenting end was bare. A considerable part of the capsule still adhered to it. The œdematous lip-like parts were ascertained to be parts of the lower edge of the investing capsule, where the tumour protruded through it.

In this case of enormous polypus, the operative difficulty did not lie in reaching or cutting the attachment of the tumour. To effect this, as a beginning of the operation, would no doubt have been almost impossible; but the operation was finished without cutting through the attachment: and separation of the polypus from its attachment at the beginning of the operation would not have facilitated further proceedings in any considerable degree. The whole difficulty of this operation lay in the great bulk of the tumour, or, what amounts to the same, in the smallness and indilatability of the orifice through which it had to be brought. We tried to pull it over the perineum, but failed. Had we succeeded, it would have been effected only with great laceration of the parts. To reduce the bulk of the tumour, then, was essential. Several plans for doing this have been proposed. Lately,

Dr Simon,* of Rostock, has practised a method which he calls "operative elongation," and which consists essentially in making, by scissors, deep incisions; cutting through the capsule transversely at various parts. When this is done, the tumour elongates under traction, and its removal can be managed. But the plan proposed and practised by Dr Alfred Hegar,† of Darmstadt, seems to me to be more simple and efficient. It consists in cutting the tumour spirally, so as to make it somewhat resemble the child's toy, which consists of a piece of ivory cut spirally, so that it can be drawn out to a great length. Both these operations are more easily described than performed. In the case which I have narrated, I attempted, as far as possible, to effect the spiral cut; and though the spiral was repeatedly broken across, the method was useful in effecting the elongation of the tumour, and its consequent diminution in bulk in those measurements which affronted the vaginal orifice.

Both Drs Simon and Hegar regard their procedures as having for their object to reach the pedicle or insertion of the tumour, in order to cut it through. But this is not the proper view to take of them. It is not always necessary to cut through the insertion or pedicle, as Drs Simon and Hegar seem to think. This case illustrates this point. Besides, when the plan which I followed in this case can be adopted, it saves a risk of grave error, into which Dr Hegar has fallen. This consists in cutting through the tumour, taking it for the pedicle; thus leaving in utero a part of the tumour, which may be a source of danger in various ways, or demand renewed interference.

The method of removing large fibrous polypi by spiral incision may be advantageously followed in the more difficult operations of removing, by avulsion and enucleation, imbedded fibrous tumours

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* Monatsschrift für Geburtsk, Bd. xx. 1862, S. 467.

† Ibid., Bd. xxi. 1863, S. 220.

Dr Keiller had seen a case of fibrous tumour with Dr Craig of Ratho, where, on examination, an enormous mass was felt through the os, and an attempt was made to assist nature in expelling it. The attempt to remove it, however, was unsuccessful, and the patient afterwards died from exhaustion. The tumour was embedded in the substance of the uterus; and, perhaps, if cutting had been resorted to in combination with traction, the result might have been successful. The specimen was preserved in his museum. He had removed one or two tumours by enucleation. In such cases, he was of opinion it would be advantageous to dilate the passages before operating; and this might be easily done by means of the india-rubber dilator. By fitting the tube of the ether spray apparatus to the bag, inflation and dilatation were readily effected.

Dr Duncan said the great things in all such operations were patience and perseverance. The chief difficulty in his case arose from the bulk of the tumour, and he thought that Dr Keiller's plan might be serviceable. In virgins, much difficulty sometimes arises from the tightness of the vaginal orifice; and cases have occurred in which the perineum was torn into the rectum.

Dr Thomson said the tumour removed by Dr Duncan was the largest he had seen. He had found Dr Keiller's bag extremely useful in many cases, but he doubted whether it would dilate the orifice of the vagina.

Dr Bell did not understand why there should be more difficulty in dilating the vaginal orifice than the os uteri.

CASE OF HYPERTROPHY OF THE CERVIX UTERI, WITH AMPUTATION.

By James Young, M.D.

November 10, 1869.

Mrs M., æt. 37, complained of great prolapsus of the uterus, causing her considerable pain and inconvenience. On examination, I found her suffering from an hypertrophied uterus, with an enormous cervix hanging down several inches between her thighs. The following were

the measurements:—From os to fundus, 5½ inches; the cervix alone measured 3¾; to the fold of the vagina the circumference of the neck was 6½ inches. Patient states that she had, 16 years ago, two male children, and that thereafter, in consequence of lifting heavy clothes boilers, she felt that she had injured herself in some way or other. She has suffered very much from the prolapsus for five years, but the cervix uteri has protruded only for the last twelve months. Since May 1869, the extruded portion had become much larger, and the mucous membrane thereof like ordinary skin, and quite smooth in appearance. I tried every kind of pessary, but all failed to give the least relief. I therefore advised her to have a portion of the neck amputated. Accordingly, the operation was performed, with the kind assistance of Drs Keiller and Stevenson Smith, on Monday, 25th October. Several points of interest presented themselves, and the mention of them may not be altogether uninteresting to obstetricians. The patient was put under the influence of chloroform, and the écraseur was applied about 2 inches from the os, and gradually tightened by a very slow movement; but as the indentation was being made, the chain of the écraseur snapped, and we were compelled to suspend the operation until Dr Keiller kindly brought one of his own instruments, and another from Sir James Simpson. We applied Dr Keiller's beautiful instrument, but foreseeing a certain risk of its breaking, we removed it, and applied a third écraseur of Sir James Simpson's, and with which we proceeded to complete the operation. Very slowly and gradually the chain became buried in the tough, dense substance of the cervix, and in about twenty minutes the chain was tightened to its full extent. At this stage Dr Keiller found it necessary to make several incisions with a bistoury before we could detach the amputated portion of the cervix. The chief point of interest to which I would call attention, is the extraordinary amount of pressure which was required to be put on the écraseur before the operation was completed, and

which may be imagined from the fact that the stalk of the instrument was completely bent.* Doubtless, this may be accounted for by the fact that the strain was all on one side of the body. I therefore much prefer the instrument with the lateral movement, where there is less chance of such an accident happening. We applied a little of the saturated sol. of the perchloride of iron and glycerine to the lacerated surface of the cervix, both before and after the removal of the écraseur. There was little or no bleeding, and the patient made a most satisfactory and rapid recovery, being up on the eighth day. No treatment was adopted after the operation, beyond the use of tepid water by injection. On examination on the eighth day after the operation, I found the cervix uteri shrunk up, and very much reduced in size. The patient now (sixteenth day) feels an amount of comfort which she has been quite a stranger to for two or three years.

Dr Keiller said that this was about the toughest cervix he had ever met with. The instrument used in the first instance was one he had frequently operated with, and, indeed, he believed it was the first écraseur ever used in Edinburgh. He had, some time ago, removed a cervix, but the result was not so successful as in Dr Young's case, and this he thought was probably owing to too much having been removed.

Dr Duncan said he had performed this operation very often, and latterly had used the écraseur. The instrument sometimes disappoints. The first case he had used it in was a lupus of the vagina, but the body of the instrument bent so much, that he had to complete the operation by cutting. In removing the cervix, there was, no doubt, a danger of opening into the recto-vaginal *cul-de-sac*.

Dr Milne had operated in two cases of malignant disease, and in one the instrument gave way; he then used the bistoury to complete the incision. Considerable hæmorrhage followed, but it was easily arrested by the perchloride of iron.

Dr Gordon said that Sir James Simpson, in operating

* This instrument was shown to the Society.

upon a tumour, had broken two écraseurs, and it was not uncommon, he believed, to have bleeding after the use of the instrument.

Dr Andrew said he had seen the écraseur used abroad, connected with a battery, with success, and no bleeding.

EXHIBITION OF DR ZIEGLER'S NEW MODELS AND CASTS.

By J. MATTHEWS DUNCAN, M.D.

NOVEMBER 24, 1869.

THE models and casts which are here shown were made by Dr Ziegler of Freiburg, under the direction of Professor Hegar. It was this latter gentleman who planned them. They are intended for the use of the student and of the teacher, and they appear to me to be likely to be of much service to science and to purposes of illustration in the class-room.

Much is already known of the mechanism of delivery, and the models are fitted to show this, and to enable us to discover more.

Models of the kind now shown have been made by Fabbri of Bologna, with whose works I am quite unacquainted; by Hodge of Philadelphia, who dwells much on their value, and figures his own specimens in his great work entitled *The Principles and Practice of Obstetrics*, published in 1864; and now by Professor Hegar.

It is only with the models of this last gentleman that I am actually acquainted, and there is, no doubt, great merit in them. They are a very praiseworthy achievement, and will be very useful. But we must not rest content with them, for there is still much to be expected from this kind of work. We need to have the homalographic system applied here, as it has been with much advantage in other departments of medicine. Sections of actual heads moulded under the influence of the violent pressure of a difficult labour should be made, as also homalographic sections of

actual pelves with the heads in them, taken in cases of natural and in cases of difficult delivery. No doubt these will be produced in course of time.

The present series consists of twenty-seven casts. These include models of the canal of the normal pelvis, sections of the canal, both oblique and direct, model sections of the fœtal head in many directions, and models of the canals of five kinds of abnormal or deformed pelvis.

The history of these models illustrates some interesting points; for instance, the difficulty of getting a good natural pelvis. To make his works, Hegar had to resort to Litzmann for a characteristic specimen of a female pelvis! He has adapted his models to the doctrine of the direct entrance of the fœtal head into the brim of the pelvis, not to that of Naegele. While this is, of course, satisfactory to me, I have, in conclusion, to say, that I look with anxiety for further researches in the mechanism of natural delivery, believing that our present views, while certainly rudimentary and imperfect, are also not without grievous flaws in the way of positive error.

Dr Keiller said that he quite agreed in the remarks made by Dr Duncan as to the necessity of studying the mechanism of labour more thoroughly than it had ever been done. Every practitioner ought to study the mechanism in every case, and in this way the obstructions which often took place during labour might be explained. Such a study tells us what no casts can ever do—it enables us to ascertain the influence of the various forces which are at work. The only objection to the casts shown, and indeed to all casts, was that they were too mechanical—they do not show the effects of pressure. He was of opinion that casts made of vulcanised india-rubber were required for this purpose.

A CASE OF PUERPERAL CONVULSIONS, WITH AMAUROSIS, TREATED BY BLEEDING AND VERSION.

By ANGUS MACDONALD, M.D.

NOVEMBER 24, 1869.

ON 16th October 1869 I was summoned, about 4.45 A.M., to see Mrs B., aged 32, residing at No. 6 New Broughton. About a year previously she had had a miscarriage at the fifth month, for which she was attended by Dr Andrew Inglis, and made a good recovery with no untoward symptoms. She was now about the full time of utero-gestation, and daily expecting her confinement. For several days past, I learnt that she had suffered from severe headaches, but these had become much worse on the forenoon of the previous day (15th October). About one o'clock A.M. she fell asleep for about a quarter of an hour, and on awakening, found that her sight was completely gone. She also felt sick, and vomited. About three o'clock she had a convulsive attack, which lasted about three minutes, and after an interval of half an hour, this was succeeded by a second seizure, which continued for a little longer. At four o'clock she had another fit, after which her husband came for me. On my arrival, I found the patient's face much flushed and puffy. Her legs, which I ascertained had been previously much swollen, were quite free from œdema. Upper lip considerably swollen, but no bleeding from the mouth nor appearance of biting the tongue. She had suffered from diarrhœa in the mornings for some days past. She was quite unconscious. Pupils dilated to the size of $\frac{3}{16}$ths of an inch, fixed and immovable on the application of light. I passed the catheter and drew off some urine, which I found was densely loaded with albumen.

On examination per vaginam, the os was found dilated to nearly the size of a shilling. The cervix was soft and

dilatable, though rather thick. Meanwhile slight pains came on at regular intervals of about fifteen minutes. I sent for chloroform, and also for the following mixture:— ℞ Potassæ acetatis, gr. 240; aceti, fl. oz. ½; aquæ ad fl. oz. vi. M. et S. Sig., A tablespoonful every four hours.

I waited by the patient till six o'clock, but as no convulsions came on, I then left, giving orders to send for me in case of a recurrence of the fits, and directions that a horn-spoon should be placed between the teeth of the patient should a fit come on during my absence, and that she should be kept from injuring herself.

At 7.50, I was hurriedly called to see my patient, who had been seized with a fit of very much greater severity than any of the preceding. While in this fit she had bitten her tongue very badly, and since she came out of it, she had become wildly delirious. I arrived at 8.15, and found the patient tossing about in a very wild and restless manner. The pains had gone completely away since half-past six o'clock. I waited with her till 8.45, when she was seized with another fit. The spasms were tonic, general, and of great severity. The head was turned to neither side, nor was one half or limb of the body more affected than the other, except that both eyes were turned powerfully to the left. I held a large horn-spoon between the teeth, and yet the patient's tongue bled a little. The face became intensely congested during the attack, and the breathing deeply comatose. So intense was the coma and congestion, that, though at first I attempted to modify the fit by administering chloroform, and though I cannot charge myself with having any superstitious fears of the evil effects of the inhalation of that drug, yet I did not administer it during the seizure, for fear of cerebral effusion. The attack passed off gradually in about ten minutes.

From 8.45 to 11 o'clock, only one pain, of no great intensity, and no fit whatever, occurred. About ten o'clock I happened to meet Sir James Y. Simpson, and mentioned the case to him. He advised, since the cerebral

congestion was so intense, to bleed from the arm, and then try chloroform. I returned to my patient a few minutes before eleven, and immediately after my arrival, a fit of still greater intensity than had as yet occurred, came on. The face was literally black, so deep was the congestion; and on recovery from this fit, an examination per vaginam revealed no change on the os, which was still little more than the size of a shilling-piece. I then at once proceeded, with the aid of my friend Mr Copland, to perform venesection, and drew fully twelve ounces from the right median cephalic vein. It was interesting to notice the change in colour which the face underwent during the extraction of blood. The flushing of the face passed almost entirely away. Scarcely, however, had I bound up the arm, than another convulsion came on; but this time it was not nearly so severe, nor was the congestion of the face nearly so deep as during the former attacks. Instead of being purple, it was only pale red. About five minutes after recovery from this fit, a pain supervened; and on examination per vaginam, during and after it, I was struck with the very remarkable and astonishing change in the dilatability and softness of the os and cervix uteri. On distending it a little with the finger, I found it yielded so readily, that I felt I could safely introduce the hand at once, and turn. Mr Copland put the patient under the influence of chloroform, and, while he did so, I kept dilating the distensible os with the hand; and by the time the patient was completely over, I found I was easily able to pass the hand through the vagina and os into the uterus. The presentation was a vertex in the left occipito-anterior position. I then ruptured the membranes, and with a little difficulty, succeeded in seizing hold of and bringing down the right foot. In spite of every effort to the contrary, however, prolapse of the cord took place at the same time. I was therefore left no alternative but to hasten the labour as much as possible, in order to save the life of the child. During traction upon the body of the child,

the uterus was pressed upon, and irritated as much as possible through the abdomen, with the view of keeping it contracted. Little difficulty was experienced till the head engaged in the brim of the pelvis, when, owing partly to a rather bulging sacral promontory, and partly to the anterior lip of the cervix getting jammed between the head and the symphysis pubis, the head became so tightly fixed (the face looked towards the left sacro-iliac synchondrosis), and I had to use so great an amount of traction in consequence, that, knowing the existence of prolapse, and feeling the cord had now ceased to pulsate, I was about to desist for a time from hurrying the labour, and to give the child up for lost, when a slight tremor of its body made me aware that it still lived. Another effort of traction pulled the head through the passages. The labour was ended about a quarter to twelve o'clock noon. The left parietal bone of the child was deeply indented with the traction of it over the sacral promontory. After the diligent application of the ordinary means of resuscitation, the child breathed well, and cried lustily. The placenta separated of itself very shortly after the delivery of the child. There was a little hæmorrhage, which quickly yielded to the usual means.

The pulse was now 120 per minute, the pupils still widely dilated and fixed. An hour after delivery, pulse 86; pupils still dilated and fixed; patient still unconscious; no recurrence of the fits.

At 5 P.M.—Patient sleeping rather heavily, no recurrence of fits; pupils still dilated, though now somewhat sensible to light. Patient had been rather unmanageable and outrageous at times, and it was only with considerable difficulty she had been got to take the medicine.

At 9 P.M.—Consciousness slowly returning; patient still quite blind; pupils getting much more sensible to light. She took from me quite readily some egg-flip which I gave her. Found, on passing a catheter, about 10 or 15 ounces of water in the bladder, which on testing was found still intensely albuminous.

17*th*, 1 P.M.—Patient very much better, but complains of feeling very tired and sore. She is unable to turn herself in bed or move her limbs. Sight now returning; pupils natural; urine only slightly muddy on being heated and nitric acid added to it. Pulse 72, of fair strength.

8 P.M.—Patient quite conscious, and sees quite distinctly. A large quantity of urine removed by catheter. Pulse 72, of fair or rather of good strength.

18*th*, 11 A.M.—Patient had passed an excellent night. Took in the morning a dose of castor-oil, which had operated well. Albumen still present, though greatly diminished.

For the two immediately succeeding days, the patient was unable to move herself when she required to evacuate the bladder or bowels, so that she needed great care to keep her clean and dry, but, on the whole, she made a good and rapid recovery.

24*th November*.—Her urine still contains a considerable amount of albumen, though, on careful microscopic examination, I failed to find any tube-casts.

OBSERVATIONS.—I have presumed to lay this case before the Society, chiefly on account of the remarkable effect which bleeding exerted upon the dilatation of the os uteri. Educated at a period when perhaps too little attention was paid to bloodletting, except to condemn it, I confess myself to have been previously a rather decided sceptic in regard to its efficacy for such a purpose. But from what I saw in this case, I feel bound to declare that, after all, we possess no other means which can cope with bloodletting in its effects upon the dilatation of the os.

It is a good example of the truth of the common assertion, that no belief, custom, or mode of treatment, ever took a general hold upon men's minds, except there was in it a very considerable amount of truth. I feel assured that bloodletting never would have had such a hold upon the profession, had its effects not been very beneficial indeed; and that, though our immediate predecessors in the medical art went too far in recommending the ab-

straction of blood, yet, from our extreme views in the opposite direction, it is a serious question whether we do not frequently deprive our patients of a very valuable therapeutic means. We are warned, it seems to me, by such cases as the present, and by many others of like nature, to reconsider our position, and, instead of condemning bloodletting in a wholesale fashion, to select proper cases for its application, and use it for these, and these only.

But besides acting beneficially as a means of relaxing the os, the bloodletting had a manifest effect in relieving the cerebral congestion, as evidenced by the change in colour which the face underwent. At the same time, it very markedly mitigated the severity of the convulsions; for the fit, which supervened immediately after the bloodletting, was not nearly so intense as the one immediately preceding it. Indeed, it was with the view of mitigating the cerebral congestion, and thereby lessening the severity of the fits, that I opened the vein in the arm.

Another peculiarity in this case was the complete amaurosis. Amaurosis is a very frequent complication of puerperal convulsions, with albuminuria, as has been pointed out by Sir James Y. Simpson, in the first volume of his Obstetrical Works. But it is usually partial, and but seldom complete as in this case. Amaurosis is, however, almost always, as in the present case, temporary. A curious case, reported by Dr Ingleby, is referred to by Sir J. Y. Simpson, in a footnote, page 825, volume I. of his works, in which puerperal convulsions occurred during the first labour, but in the subsequent confinements their place seemed to be taken by complete amaurosis, which lasted the whole period of labour. The blindness, occurring as a complication of albuminuria, is not usually accompanied by any structural change in the eye or its appendages.

Headache is also a most common and most troublesome symptom, and is accompanied with more or less complete loss of consciousness. It is curious that, occa-

sionally, before convulsions really come on, there may exist such a disordered condition of the intellectual powers, that the patient has no recollection whatever of what has taken place, although she speaks and acts quite naturally, and complains only of a severe headache. I recollect attending a case five years ago, when acting for Dr Bryce in Dalkeith, which forcibly impressed this upon my mind. The convulsions came on for the first time an hour fully after the labour had been completed, and there was no œdema or other sign to suggest the likelihood of albuminuria. She, however, complained very much of intense headache during the whole of the period of parturition, and had a good deal of that tendency to shiver, which are so frequently found in ordinary labour cases. But on being interrogated after recovery from the convulsions, she had no recollection whatever of anything that happened during the whole of her labour, though all the while she seemed to me to act and talk quite rationally.

As to the cause of those terrible convulsions accompanied with albuminuria, on consulting the authorities, the state of our knowledge seems eminently unsatisfactory. The theory of urea in the blood seems without any good foundation; the same also must be said of Frerich's theory of carbonate of ammonia resulting by fermentative change from the urea of the blood. That it should be due to a poison existing in the blood in all cases, seems to be strongly negatived by such as yield readily to bloodletting, For, if a poison existed in the blood to such an amount as to cause convulsions of such terrible intensity, it is unreasonable to suppose that the fractional part of it eliminated in ten or twelve ounces of blood could have any beneficial effect on the general result. Neither will the theory of vascular pressure or tension alone suffice as a cause, for we so frequently see the very worst cases of so-called uræmic convulsions occurring, as in this case, without dropsy to any extent. I have myself treated

other two cases of exceedingly severe uræmic convulsions, and in neither of them was the amount of dropsy other than exceedingly trivial. The pressure in some way or another must, nevertheless, have some relation to the convulsions, else how could bloodletting be so markedly beneficial as it is often found to be? It may be, as has been suggested, that the proper elimination of nerve-tissue waste so interferes with the vital activity of the nervous system as to render it specially supersensitive for the time being, and that a small amount of pressure added to this condition is capable of producing those terrible disturbances in the nervous system, under the action of comparatively slight exciting causes, such as the contraction of the gravid uterus. Be this as it may, however, the irritation of the contraction of the gravid uterus seems to increase the tendency to the fits very much, and to warrant us in endeavouring to hasten the completion of labour by all legitimate means.

Dr Pattison said that in his younger days bleeding was much practised in cases of rigid os, and he had seen the benefit of bloodletting in cases of convulsions.

Dr Keiller said that one of the first cases he was called on to attend was one of convulsions. He bled freely, and was satisfied that the patient's life was saved in consequence, and he believed that nowadays we did not bleed often enough.

Dr Murray mentioned a case in which, after puerperal convulsions, one eye was much damaged, and sight was never recovered. The patient had albuminuria.

Mr Pridie had met with some seven or eight cases of puerperal convulsions, which all recovered after bloodletting. In one of the cases there was complete amaurosis.

Dr Thomson said he had seen a great many cases of convulsions, and he believed that in many cases the hæmorrhage which followed delivery was an important element in the treatment. In convulsions he was in the

habit of giving the bitartrate of potass in teaspoonful doses every hour.

Dr Bruce said he had not met with any case of puerperal convulsions for some time. He relied very much upon the good effects of bleeding, and his cases had all done well.

Dr Cairns thought the profession was pretty much at one in regard to the propriety of bleeding in such cases, and its beneficial influence might be explained in this way, that the system could bear some amount of poisoning, but whenever a certain point was passed convulsions took place, hence the relief afforded by bleeding even in small quantity. In rigidity of the os he did not think bloodletting of much consequence after all.

Dr Bell said that bleeding had a marvellous effect in dilating the os. It used to be common to bleed to syncope, and he had never seen any evil effects from it.

Dr Duncan said he agreed with the general tenor of the paper. He remembered when there was nothing else but bleeding in cases of convulsion; then chloroform took its place. But there was another method which he had seen used with success, and that was simply doing nothing at all. He had seen a large number of cases, and was of opinion that in one set of cases, where there was much lividity of the face and unconsciousness bleeding was advisable, and chloroform was the reverse of useful; but in another class of cases, where the fits were violent and frequent, chloroform was beneficial. In other cases, again, doing nothing was useful, especially where labour is going on quickly.

Dr Pattison wanted to know if Dr Macdonald had given the bromide of potass in the case he had related. He thought it would have had good effect. He had seen its good effects in *delirium tremens*. In convulsions he had lost one case in four.

Dr Macdonald replied, he did not think the bromide of potass would have been of use whilst the uterine irritation existed.

Dr Young stated he was called to a case in which, when he saw the patient, the os was the size of a shilling-piece. From 11 P.M. to 6 A.M. convulsions came on every half

hour, and continued after delivery. The patient was leeched and blistered. The urine was also found to be albuminous. The patient died the same night. He questioned whether general bleeding would have been useful. There was no unusual amount of discharge.

CHLOROFORM INHALER.

INVENTED BY DR ELLIS, of London.

DECEMBER 8, 1869.

DR JAMES YOUNG exhibited a chloroform inhaler, which had been invented by Dr Ellis, of London.

CASE OF A CHILD BORN WITHOUT AN OCCIPITAL BONE.

BY CLARKSON CUTHBERT, M.D.

DECEMBER 8, 1869.

THE following case, being one of an unusual character, has induced me to bring it before the Society :—

On the 18th of October 1869, Mrs M'L. was delivered of a boy at the full time, her eleventh child. All her children were healthy and well developed, with the exception of two, viz., the ninth, which wa s still-born and premature; and the eleventh, the subject of the present remarks. My friend, Dr Menzies, attended my patient in her last confinement, and he was rather puzzled to make out the presenting part. His first examination led him to believe that it was a breech case, but the presenting part felt too soft for that. Then his next impression was that he had to do with an hydrocephalic head. The speedy termination of the labour, however, soon cleared up all doubt.

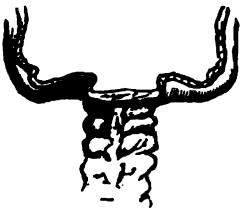
At the birth of the infant, the part marked A had a

somewhat fleshy appearance, and membranous. The parts marked B were only slightly bulging; the extreme

FIG. 1.

FIG. 2.

bulging, evidently caused by pressure of the brain against the scalp, did not take place till the second day. The membranous part at this date began to dry up, and it ultimately became quite black, except a small part which contained a quantity of fluid.



On examining the back of the head, I could find no occipital bone; the head, as far forward as the dotted line C in the second illustration, was quite soft, apparently from brain matter, and unprotected by bone. I was afraid to push my inquiries very energetically into the base of the head, as, on making pressure, the child began crying from pain. But, so far as I could ascertain, the spine had an abrupt termination, as represented in the diagram.

After the death of the infant, which took place on the eleventh day, I was allowed to make a more minute examination, which I did by pushing my finger well back into the neck, and tracing the edge of the bone till it joined the lowest part of the line C. The mother would not permit me to open the body.

The parietal bone did not extend further back than the line C. The edge of the bone passed slantingly down

towards the angle of the jaw, where it joined the ridge of bone coming from the superior end of the spine. The frontal bones were normal, and the body was in every other respect healthy and well developed.

The expression of the face was very peculiar; the right eye had a slanting position, which gave it an Asiatic cast. This peculiarity was very much modified by the eighth day; the features then assumed more the ordinary appearance of infancy; the skin also became more pinky in colour.

The infant could not suck its mother's nipple. I examined the mouth with the view of ascertaining whether there was any defect, but there was great difficulty in opening the mouth; the jaw seemed as if partially anchylosed to the temporal bone. I could discover no defect. A nursing-bottle was provided, the nipple of which it sucked pretty well. It was also fed on sack whey.

I may mention, in connexion with this case, that, on the morning of the eleventh day, convulsions came on, which continued more or less all day, and the child died that night.

Dr Keiller said, it appeared to him the case was akin to spina bifida. He had a drawing not unlike that shown. His case is reported in the minutes of the Society. He thought it was a cystic tumour, and tried to operate. He opened the tumour, and a quantity of fluid escaped. The cyst was double, as in these cases it usually is. Dr Cuthbert's case is one of incomplete development, occurring in the mesial line, as these defects generally do, both anteriorly and posteriorly. Cases of this kind generally die of convulsions. In the anencephalia the anterior part of occipital bone is generally present, and the case detailed to the Society is a modified form of the same condition.

AN AID TO PARTURITION, &c., BY A NEW MECHANICAL APPLIANCE.

By Protheroe Smith, M.D. of London.

(*Communicated by James Young, M.D.*)

December 8, 1869.

Dr James Young read a communication, entitled "An Aid to Parturition and to the Treatment of Displacement of the Uterus, by a New Mechanical Appliance," by Protheroe Smith, M.D. of London.

The instrument was exhibited to the Society, and several fellows made remarks on it.

SHORT UMBILICAL CORD.

By James Young, M.D.

January 12, 1870.

Dr James Young showed an umbilical cord, measuring only ten inches in length. He had consulted various books on the subject, but could find no instance related where the cord was so short, and Sir James Simpson, who examined it, said it was the shortest he had ever seen. The child was in such close contact with the mother, that Dr Young had considerable difficulty in getting the ligature applied to the cord.

Dr Cochrane said he had met with a case of short cord, in which the labour was rapid, but the cord gave way near the navel. The child did well.

Dr Bell said that the usual opinion was, that shortness of the cord was a cause of tedious labour, but in Dr Young's case it did not seem to have been so.

Dr David Gordon said that, in a case of so much shortening of the cord, the placenta would be drawn out of the uterus.

Dr Cairns said that, unless the placenta had been detached, he could not conceive how the labour could be so rapid as it was in this case.

ON A CASE OF OVARIAN DROPSY.

By R. Peel Ritchie, M.D.

January 12, 1870.

The subject from whom this preparation was removed after death was Miss M. O——. Although of respectable parentage, she became dissolute in her habits, and for many years led a profligate life; but, having become legally insane, she was placed, in 1856, being then in her thirty-eighth year, in Bethnall House Asylum, London, where she was under my care whilst medical officer in that institution. She was very violent, noisy, and obscene, intractable in disposition, and filthy in her language, and habits. To examine her was a matter of some difficulty, and it was owing to her peculiar mental condition that she had been suffering from ovarian tumour for some time before the exact nature of the abdominal enlargement was ascertained. In, and for some time previous to, March 1866, there was such an increase in size that her condition then received more minute attention; and during the succeeding year, as she continued to enlarge, she was transferred, in April 1867, to the infirmary department of the asylum, not only on account of her enormous size, but for her general debility and dirty habits. Under the treatment of Dr. Millar, the superintendent of Bethnall House,—to whose kindness I am indebted for this preparation and the notes of the case,—various palliatives were tried, but in the circumstances operative interference was not considered advisible. Her circumference at this time measured fifty-four inches, and the distention of the abdomen extended upwards to the xiphoid cartilage. At this time her state was so feeble that her death was almost daily expected. On the 19th of May an improvement in her condition is recorded in the case-book, to the effect that the measurement was reduced to fifty-one inches, and that the urgent symptoms which had led to her removal

to the infirmary had disappeared. The next entry I quote is that of the 4th of September, when it is stated that she passed urine involuntarily as she sat about. On the 9th of the same month, the night attendant reported that for the last two nights she had passed immense quantities of urine, and was much reduced in size. The entries of the 13th and 19th still record her decreasing bulk, and on the latter date the measurement was forty-one inches. During the next month the decrease went steadily on until October 19th, when the circumference was reduced to thirty-eight inches.

Dr Millar remarks that, on account of her very dirty habits, it was not possible to ascertain where the water came from; and her language was so incoherent that no information could be obtained from herself.

After this her size began again to increase, and by the 5th of November (or in sixteen days) she had enlarged by two inches—that is, to forty. Whilst this increase in size was progressing, a significant entry appears on the 2nd of January, 1868—that she has not been wet since the dropsy disappeared. This, however, did not continue, for, on the 23rd of February, it is stated she is wet every night, and that her circumference had nevertheless enlarged to forty-five inches. During the four succeeding months a steady increase went on till the 26th of June, when her girth was forty-nine inches. On the 16th of July the report is to the effect that the dropsy had again nearly disappeared; and on the 25th of July the smallest measurement is recorded: it was only thirty-five inches.

From this date she again enlarged, and till the time of her death a steady but gradual increase was noted. By March 1869, she had enlarged to forty-five inches; in April, to forty-seven; in May the measurement was forty-eight; in June and July the circumference was fifty inches; in August the girth was fifty-one, and in September fifty-one and a-half inches. After this she gradually sank, and died on the 23rd of November, after bilious vomiting, in her fifty-first year. The greatest girth after

death was fifty inches and a-half, and the measurement from pubes to end of sternum was twenty-three inches. The measurements taken during life were when she was in the erect posture, that after death in the recumbent.

Dr Millar states, "I carefully opened the peritoneal sac and exposed the cyst, which was like an enormous bladder, and adherent all around, but very slightly to the peritoneum. The posterior part of the cyst was quite transparent; the fluid could be seen moving in it. The lower part of the cyst, particularly on the right side, had numerous large vessels running round it. The fluid was syphoned out, and amounted to five gallons three pints; it was limpid, quite clear, highly albuminous, and of 1015 sp. gr. I looked to see where and how the water could have disappeared, but could not satisfy myself. At the point apparently most likely I could not trace any opening. There was no fluid in the peritoneal cavity."

The case I have just read is interesting in two aspects, its psychological and its obstetrical.

The immoral conduct of the woman before she became legally insane, is of interest in connection with the ovarian disease which subsequently became developed. Whilst living her profligate life, I hold she may be fairly considered to have been morally insane. Her removal to an asylum would have been justifiable, and to have placed her under restraint would have been a kind act to her. Unfortunately it is not easy in all cases to decide between moral insanity and responsible immorality.

Her obscene language and gestures, when there was no longer doubt as to her general insanity, also point to the ovarian irritation which existed. The connection between ovarian irritation and mental alienation is generally recognised, and the English law courts have apparently admitted the influence of disordered menstruation in producing moral insanity. The case to which I allude is that of Regina *v.* Brixey. On that case Dr Taylor remarks, "The prisoner was acquitted on the ground of insanity, pro-

bably caused by obstructed menstruation."* That ovarian irritation in relation to mental disease does not, however, invariably produce insanity of the nymphomanic type is also well known, and delusions of other kinds are frequently met with.

I have had one other case of ovarian dropsy in the insane under my care, and in that case the tumour in the left iliac region preceded the mental disease by a considerable time. The patient was melancholic and suicidal. She believed she was surrounded by thieves and nasty boys, who tried to make her a thief too. I might mention various authorities on this subject, and, amongst others, Tilt on Ovarian Diseases, Seymour, and Conolly occur to me.

In the case before us it may be questioned whether the peculiar ovarian condition was the cause of the type of the insanity, or was the immoral life the cause of the ovarian disease? I have yet to learn that ovarian dropsy is a sequent to immorality, although Dr Keith informs me its post-nuptial manifestation is not infrequent. And in reply to the first query, I would state there was good reason to believe in a hereditary tendency, and it is possible that, in a female so predisposed, the ovarian irritation may have acted as a developing cause of the mental disease. After ovulation has commenced, it is not easy to say at what time these morbid cystic formations in the ovaries begin; and it is not improbable that they may have existed in a condition likely to produce ovarian irritation without assuming, at least for a time, that more active state of growth when they become evident to sight and touch. These cysts do not all grow with equal rapidity. This is well shown in the specimen before me; for whilst it may be called unilocular, in so far as only one cyst has assumed the character of an ovarian dropsical tumour, still the presence is noticeable of various cysts of—shall I say—rudimentary size. It is probable, therefore, I consider, that ovarian irritation had existed for many years.

* *Medical Jurisprudence*, 6th ed., p. 926.

In its obstetrical aspects the case is also interesting. The first distinct notice of her true condition is in March 1866; but long previous to that date her peculiar appearance had attracted notice, and it is not impossible she had been even self-conscious of it. She used frequently to say she was in the "family-way"—an observation which, at the time (no examination having been made, for the reason already stated), was supposed to be merely connected with her nymphomaniac state.

In April, 1867, her condition was one of great urgency, and seems to have been connected with the ovarian tumour. It gives a starting-point as to her size, her girth at that time being fifty-four inches. The interesting point too is, that her state at this time apparently indicates the commencement of the first reduction in size; for during the next month she became three inches less in circumference. In September the reduction was more rapid, for during that month there was a decrease of ten inches. During the following month the reduction in size amounted to three inches only. In the course of five months there was consequently a decrease of sixteen inches. It is of importance to note that in September, when the rapid decrease took place, the only condition noted as peculiar was, that she passed immense quantities of urine. Had there been, therefore, any injury to the cyst, some constitutional disturbance would have been produced; whereas the only effects observed were the diuresis and decrease in her size. For eight months she again went on increasing in bulk, until, on the 26th June, she had reached the girth of forty-nine inches, or eleven more than in October. The second diminution in size occurred in July. It was very rapid, and in the course of a month she decreased fourteen inches. From this time till her death she gradually increased, but never appears to have attained the circumference she had when first measured in April 1867.

This diminution in circumference having occurred twice may form the subject of considerable speculation, and various theories to explain it may be advanced.

The first point in the inquiry, it occurs to me, is to settle whether there were more cysts than one? At first view I was inclined to suppose there had been more than one cyst, and that one of them had ruptured on each of the two occasions alluded to; but, after making a minute examination of the preparation, I could not satisfy myself as to there being any traces of a ruptured cyst. One part, where the walls of the tumour are thickened, might, I thought, possibly be due to the altered remains of a ruptured cyst; but when I considered that the cysts must have been of great size when they ruptured—for the decrease in circumference was, in the one instance, sixteen inches, and in the other fourteen inches—I felt satisfied the small thickened portion could not represent a collapsed cyst of the required size. My speculations were, therefore, limited to the existing cyst; and the conclusion I arrived at was, although there are numerous traces of rudimentary cysts, that one only had developed to a great size. Regarding, therefore, the cyst exhibited as the only one from which the fluid had come, the next question that occurred was,—How did the fluid escape from it? After careful examination of the preparation, I could detect no evidence of a fistulous opening into the genital tract; and had there been one into the intestinal canal, it could not have been overlooked by Dr Millar, who made the post-mortem examination. On trying to pass a probe from the uterus through the Fallopian tubes, I failed to do so along the left, that being the side from which the tumour grew, but, after several attempts, a fine piece of whalebone was passed along the right. This, therefore, was still patent.

Supposing rupture had occurred, there being no trace of fistulous opening, and only the right Fallopian tube being pervious, the question is reduced to this,—Could a tube of so small a lumen that an ordinary probe cannot be passed along it, and in which there is no evidence of previous dilatation, permit of such a flow of fluid along it as to allow of the rapid diminution which took place, especially on the last of the two occasions? In my own mind I am

satisfied it could not. The only remaining way in which the fluid could be eliminated was by the kidney; and I can see no other way in which it could be removed. Sir J. Simpson, in his paper "On Inflammatory and Non-Inflammatory Ruptures of Ovarian Dropsical Cysts," very well points out how a cyst may be ruptured, and the fluid thus removed from the peritoneal sac; and Dr Graily Hewitt observes: "When the fluid escapes into the peritoneal cavity, excessive diuresis generally occurs, and the size of the abdomen lessens." Such an accident having happened, the removal of the fluid by absorption and elimination by the kidney is well known and generally recognised. But these observations are not applicable to the case before us; for in the history there is no reference to such a train of symptoms as accompany the sudden rupture of a large ovarian cyst and the extravasation of its contents into the peritoneal cavity, and what is even more important, there is no trace in the walls of the cyst of rupture having occurred. At two places I had some hesitation whether the appearances presented might not be due to cicatrices; but my conviction that they were not was confirmed by Dr Thomas Keith, our distinguished ovariotomist, who kindly examined the preparation. He informs me that such appearances are not uncommon in cysts where there has been no supposition of rupture having occurred; and he is also of opinion that no rupture in the present instance had occurred. He has seen several cases of a somewhat similar kind to that of Miss M. O——, and had observed the decrease of size and its subsequent increase. He was further of opinion there would have been collapse of the cyst had rupture occurred.

The conclusion to which I come in this interesting case is, that the fluid had been in some way absorbed from the cyst directly, and without the intervention of rupture. Dr Keith further informed me that in the cases in which he had observed a similar decrease the fluid was clear and thin. This condition subsisted in Miss O——'s case, for Dr

Millar states that the fluid was limpid and clear, and of 1015 specific gravity.

It is possible, and I may even say probable, that an endosmotic change had taken place by means of the large vessels, to which allusion is made in the post-mortem appearances; and such was favoured by the extreme thinness of the posterior wall of the cyst, which was quite transparent, and by the limpid character and low specific gravity of the contained fluid. In such a view, it would be expected that when the distension of the cyst was greatest, the walls therefore thinnest, and the surrounding bloodvessels most stretched, the transmission of the fluid would most likely commence. Referring to the case, it will be observed that the decrease in size in the first instance began when the greatest bulk had been attained, and that the increase commenced when the reduction had reached sixteen inches; and in the second instance, a more rapid reduction than in the first occurred when the distension had reached forty-nine inches, and when the lowest measurement of thirty-five inches was arrived at the cyst reversed the action and again filled. It will be instructive to have my view on the one hand confirmed, or, on the other, to have a more satisfactory explanation of the phenomena.

I have only further to add that I understood Dr Keith to say he had rarely seen so small a pedicle, and that the case would have been an excellent one for operation. Had the unfortunate patient's mental condition been different, she might possibly have had the benefit of his skilful interference.

A statement of variations in size is subjoined:—

1867.	April		54 inches (maximum).		
	May 19	= 51 „	minus 3		
	Sept. 19	= 41 „	— 10	— 16	
	Oct. 19	= 38 „	— 3		
	Nov. 5	= 40 „	plus 2		
1868.	Feb. 23	= 45 „	+ 5	+ 11	
	June 26	= 49 „	+ 4		

1868.	July 25	= 35 inches	minus	14	— 14
1869.	March	= 45 „	plus	10	
	April	= 47 „	+	2	
	May	= 48 „	+	1	
	June	= 50 „	+	2	16½
	July	= 50 „			
	Aug.	= 51 „	+	1	
	Sept.	= 51½ „	+	½	
	Nov. 23	= 50½ „	post-mortem circumference.		

Dr Duncan said, that in his opinion one of the most interesting points in connection with the history of Dr Ritchie's case was the rise and fall in the dimensions of the cyst, and the account given was of value as showing that diminution might take place in various ways. The tumour may become dormant, for instance, the greater part of the fluid becoming absorbed, and what is left transformed into a putty-like substance, resembling what is seen in old abscesses. Post-mortem disclosures prove that this is one way in which a cyst may diminish in bulk. In the second place, it is alleged by some that a large ovarian tumour may be absorbed, cyst and all. Dr Duncan does not believe that such a thing ever took place. Thirdly, cysts may diminish in size by their walls giving way. He remembered a case in which the cyst burst twice, and the belly collapsed, and the woman suffered no pain. There is a statement made by authors, which has not, however, been verified—namely, that a permanent opening may remain in the wall of the cyst. It is no doubt true that in post-mortem examinations and ovariotomies, fluid is frequently found in the abdominal cavity, which is the same as that contained in the cyst. This fluid probably comes from the cyst, but he knows of no preparation which demonstrates this. Then, again, a cyst may empty itself by communicating with a mucous channel. Dr Duncan has known a large cyst burst high up into the bowel, and discharge gallons of fluid without producing any diarrhœa. It passed into the colon and became absorbed. It is not necessary, therefore, to have physical evidence of such rupture of a cyst. The same remark is true in regard to diuresis and diaphoresis. He was of opinion that the first explanation would account for the increase and decrease in the size of the tumour in the case related by

Dr Ritchie. Dr Duncan has put on record a case where the fluid was discharged by the vagina, though how it got there no one could say. The patient suddenly found herself wet while in bed one day, the tumour collapsed, and she became "neat," as she expressed it. The cyst refilled, however, and she was operated on, but no adhesion of the cyst was found to any part of the genito-urinary organs; perhaps in the re-growth of the tumour, the adhesions were broken up, and no trace of them left.

Dr Bell asked whether the previous history of the case might not lead to the supposition that the increase and decrease of size was dependent on ascites?

Dr Ritchie said that no fluid was found on examination in the peritoneal cavity.

Dr Burn was of opinion that, had ascites been present, it would have been diagnosed. He had had a case in which the cyst ruptured, and the woman suffered greatly from peritonitis. She is now, however, very well, and the tumour has not refilled.

TWO CASES OF PLACENTA PRÆVIA, THE FORMER TREATED BY COMPLETE SEPARATION, THE LATTER BY VERSION.

By Angus Macdonald, M.D.

January 26, 1870.

Case I.—On Tuesday forenoon, 27th July 1869, I was called to see Mrs ——, G—— Street, who had been seized that morning with a severe flooding while she lay quietly in bed. She stated, that on perceiving the blood come gushing from her, she started to her feet in alarm, and that the discharge produced a large pool of blood upon the floor. She was pregnant, and, to the best of her judgment, about the end of the seventh or the beginning of the eighth month of the period of utero-gestation, and expected her delivery about the end of September or the beginning of October. It was her seventh preg-

nancy, and her labours had all been natural, nor had she ever previously suffered from any of the accidental complications of pregnancy. Up to this date, during her present pregnancy, she had observed nothing abnormal except a feeling of unusual fulness throughout the iliac and the hypogastric regions. At this period she was in the possession of excellent health.

On examination per vaginam, I found the cervix soft, the os somewhat patulous and distensible, but not sufficiently dilated to admit my forefinger. There was no longer any hæmorrhage. The presentation was manifestly a vertex. But the head appeared to be high up, and the cervix, when felt, seemed separated from it by a soft, cushion-like mass, which occupied the upper portion of the pelvis. The fœtal heart was distinctly audible on the left side, and was of good strength, and regular. The mother's pulse was good, beating at about 72 per minute, and showing no signs of being affected in character or strength by the loss of blood. I at once suspected I had to deal with a case of placenta prævia; but as the state of the os forbade the possibility of my making the diagnosis complete, I ordered my patient to remain quiet, either in bed or on a sofa, and to take at the end of every six hours a tablespoonful of a mixture containing fifteen minims of diluted sulphuric acid. As yet no pains had been felt. There was no recurrence of the hæmorrhage till Friday (30th) forenoon, when it again came on, but this time in much less amount. On Sunday (1st August) I was called out of church during the forenoon service to see my patient, who had experienced a very severe recurrence of the hæmorrhage about half-past 10 o'clock A.M. I arrived about 11.45 A.M., when I found my patient in bed, with the hæmorrhage still going on rather profusely at intervals. There were now slight pains felt from time to time in the abdomen. The cervix was dilated to about the size of half-a-crown. The placenta was felt as a soft and spongy mass, covering the whole os, and attached all round the cervix. I sent away for

some solution of the perchloride of iron in glycerine and a fresh sponge. A piece of the sponge of suitable size I first steeped in iron solution, and then introduced it fairly inside the cervix through the os. This had the effect of stopping the hæmorrhage almost entirely all that afternoon and evening. The patient meantime continued to take the astringent mixture, and the room was kept strictly cool, while a cool regimen was enforced, and the patient restricted entirely to the supine position. About 11 o'clock P.M., the hæmorrhage beginning to be again rather free, I called on Dr Cappie to inform him of the case, and to secure his assistance in case I required to interfere actively during the night, owing to any sudden increase of the bleeding. Dr Cappie kindly promised to be at my call if I should need him. During the whole of Sunday night and the following Monday the hæmorrhage went on slowly on the whole, but with occasional exacerbations. The os was gradually dilating, and the sponge which I had introduced on the Sunday was expelled along with clots on the Monday afternoon. By occasional auscultation, I ascertained that the child's pulsations were gradually becoming feebler; and as by 9.30 P.M. on Monday the continuous hæmorrhage was beginning evidently to tell on the strength of my patient, and seeing that, notwithstanding the shortness and irregularity of the uterine contractions, the os was now so far dilated that I thought the hand would pass through it, I determined that it was now full time to interfere actively, and accordingly sent for Dr Cappie. He arrived at about 10.15 P.M., and he fully agreed with me in thinking that the safety of the mother would not admit of further delay. Seeing also that the partially-dilated cervix, greatly obstructed as it was by the large placental mass which occupied it, though it would admit the hand, presented so manifest obstacles to speedy delivery as to render it very problematical whether it would be possible to pull the child through it alive; and seeing, moreover, that it was exceedingly doubtful if the child was as yet viable, while

it was certain from auscultation that it was becoming very weak, we resolved to give up all attempts at saving the life of the child, and to separate the whole of the placenta from its uterine attachments, with the view of stopping the hæmorrhage, and thereby saving the mother from further risks. Dr Cappie accordingly put the patient deeply under the influence of chloroform, after which I introduced my hand with some difficulty through the os, insinuating it between the cervix and the placental mass. At this time the bleeding was very free. I now ascertained that the placenta, though attached round the cervix in all directions, extended more especially backwards and upwards along the posterior wall of the uterus, in which direction its attachment reached almost to the fundus uteri, whilst anteriorly its attachment covered from one to two inches in front of the junction of the placenta with the dilated cervix. I carefully and completely peeled the placenta off from the whole of its attachment to the uterus; and whereas the bleeding had previously been very profuse during the whole of the time I was operating, the moment I had completed the entire separation, the bleeding absolutely ceased. This took place before the membranes were ruptured. To make more certain of this fact, I asked Dr Cappie to examine and ascertain and test the truth of the observation. I then ruptured the membranes, after which the pains became in a short time both regular and powerful, and the labour was safely completed by unaided uterine efforts at 1.30 A.M. on Tuesday morning, the 2d August. The patient made a good and rapid recovery, and was able to leave for the country within three weeks after her confinement.

CASE II.—On Saturday evening, 8th January 1870, I was asked by one of my pupils at the New Town Dispensary to see Mrs Gormly, residing in Scott's Close, Cowgate, who had been suffering from uterine hæmorrhage. The patient's age was 42, and the present was her fifteenth pregnancy, but four of these, viz., the third, fifth, seventh,

and ninth, had been miscarriages, occurring about the third month. My pupil had been called to see this case on Tuesday morning, the 28th of December 1869, when he was informed that during the night she had been suddenly seized with profuse hæmorrhage, but had felt nothing like uterine pains. The patient was rather excitable, and he failed to get her persuaded to allow him to make a digital examination. He accordingly left, after enjoining her to keep quiet in bed.

On the 29th, on examination per vaginam, he felt the os sufficiently patulous to allow the point of the forefinger to enter it for some distance. He was able also to make out that the presentation was a head, but failed to discover any portion of the placenta. For the previous twenty-four hours, the amount of bleeding had been very trifling. He ordered the following mixture:—℞ Acidi gallici, gr. xx.; Acid. sulph. dil., ʒi.; Tinct. opii, ʒss.; Aquæ, ℥iv. M. Sig., "A tablespoonful every three or four hours, when the bleeding appears." From this date up to the afternoon of the 7th of January 1870, only slight bleedings occurred at intervals of about two days, but no uterine pains were perceived. During this period the patient had repeatedly remained out of bed during the greater part of the day; and on one occasion had walked to the Grassmarket and back again, a distance of fully two hundred yards. On the forenoon of Saturday, the 8th January, the pupil, on visiting his patient, found that there had been a sudden gush of bleeding the previous afternoon, which, however, ceased as soon as the patient lay down in bed. Beginning to feel rather uneasy about those repeated bleedings, he asked me to see the patient. First ascertaining that the hæmorrhage was still going on, though not at all profusely, that the placental souffle was loudest over the lower third of the abdomen, that the fœtal pulsations were strong, about 120 per minute, and that she was, as she judged, at her full time, we put her under the influence of chloroform, so as to facilitate the digital examination, as she was in a highly irritable and

nervous state of mind. On examination per vaginam, the os was discovered to be dilated to from one and a-half to two inches in diameter, and during the exploration, a slight contraction was felt which slightly distended the membranes. The os was high up in the pelvis, the cervix thick, but soft and dilatable. About one half of the area of the dilated os towards the left, and backwards, was found to be covered with the edge of the placenta, which was distinctly felt attached to the bag of membranes, and to be separated into two nearly equal portions by a fissure in its substance, passing from before backwards. As the hæmorrhage was not great, the uterine contractions were almost entirely absent; and as the os uteri was not wholly occupied with the placenta, I deemed it very likely that, as the head came down, it would entirely arrest the hæmorrhage, and that consequently there would probably be no occasion for interference. The patient was enjoined to keep strictly to the recumbent position, and having asked the pupil to send for me should either labour-pains fairly set in or hæmorrhage supervene, I left. He waited with the patient till 12 midnight, but as neither hæmorrhage nor labour-pains recurred, he likewise returned home. We saw her again at 11 A.M., on Sunday, 9th January, but found that neither pains nor bleeding had recurred during the night. We also called again at 9 P.M. same evening, but found that a small clot and a slight trickling of blood had been all the hæmorrhage which had appeared since morning. I recommended that the opium should be kept out of her astringent mixture, in case it should in any degree retard the supervention of uterine contractions.

Monday, 10*th Jan.*—Neither bleeding nor labour-pains had occurred up to the time she was visited by my pupil.

Tuesday, 11*th.*—My pupil was told, on visiting her about mid-day, that the membranes had ruptured about 7 o'clock P.M. in the previous evening. But there had been little or no hæmorrhage, though the uterine contractions had been pretty regular and strong. He then informed me of the state of matters, and I saw the patient about 4 o'clock P.M.

The os was then well dilated, but the placenta was being moved forwards to the right with the force of the pains, which evidently rather inclined to come down in front of the head, which still remained high up; so that now fully two-thirds of the area of the dilated os was covered with the placenta, instead of barely one-half of it, as on the former examination. The membranes had ruptured, entering near the extremity of the fissure in the presenting portion of the placenta; and at the seat of the rupture, as well as backwards, lying immediately behind the fissure in the placenta, the cord could distinctly be felt pulsating. There was, from time to time, considerable bleeding with the pains occasionally, but not with every pain, nor yet to such an extent as to cause the slightest anxiety regarding the patient. I accordingly waited with her till 5.30 o'clock P.M., when, on examination, I found that there had occurred a very extensive prolapse of the cord; and seeing that the cervix was sufficiently dilated to allow of turning, I felt convinced that version presented the fairest chance of effecting delivery with safety to the child; therefore, without wasting time in endeavouring to replace the cord, which still pulsated strongly, by altering the position of the mother, or otherwise, I had the patient put under the influence of chloroform; and pushing up the cord into the interior of the uterus, as I introduced my hand past the anterior edge of the placenta, I seized the right foot of the child (the back of the child was turned towards the right of the mother's spine), and brought down the breech so as to engage in the pelvis without involving any loop of the cord. Finding, by the pulsations of the child's anterior tibial that its circulation was in no way impeded, and there being no hæmorrhage, the pains were allowed almost entirely unaided to slowly push the breech and lower portion of the fœtus through the passages. After the child was thus far advanced, it became evident that the cord was being somewhere compressed, and the labour was hastened. The child did not breathe well for some little time after delivery, but ultimately came round and screamed loudly.

Some clots of internal hæmorrhage came away immediately after removal of the fœtus. The uterus contracted well after delivery, and the placenta separated a few minutes after the child was expelled. There was no post-partum hæmorrhage. During the operation of turning, the pulse rose to 130 from 110 per minute, at which it was beating before we commenced active interference; but by 12 o'clock midnight it had again fallen to 110. Both mother and child did well.

OBSERVATIONS.—Placenta prævia is admitted on all hands to be one of the most dangerous accidents with which the obstetrician is called to deal, and the importance of the subject affords, I hope, sufficient excuse for my taking up the time of the Society with these two cases. A complication of parturition, which results in the death of from one in three to one in three and a-half of the mothers who are unfortunate enough to be the subjects of it, ought, if possible, to be well understood, and at any rate be made the subject of frequent discussion among accoucheurs, for in the multitude of counsellors there is wisdom, and nothing so greatly tends towards the elucidation of truth as dispassionate and earnest discussion.

Now, the two cases just recorded seem to me to be wonderfully typical cases—the first of severe and complete, the second of mild and partial placenta prævia. The first case was marked by severe flooding, commencing at least two months before the natural period of utero-gestation, inducing the supervention of labour-pains prematurely; and, in consequence of the serious risk to which the mother's life was exposed, necessitating active interference. In the second case, labour did not supervene till nearly the termination of the ordinary period of utero-gestation, was accompanied by only comparatively slight hæmorrhages, and, but for the unfortunate prolapse of the cord, seemed likely to terminate, if left to the efforts of nature, unaided, with complete safety to both mother and child. But the former case is chiefly important on account of the confirmation which it gives of the value and efficacy

of the mode of treatment by complete separation of the placenta, recommended firstly by Sir James Y. Simpson, and defended with such ingenuity, ability, and such an array of stern facts in his articles on placenta prævia in the first volume of his Obstetrical Works. Obstetricians may dispute as to the origin of the hæmorrhage, and as to whether it is arterial or venous, as long as they please, but I can imagine no one who has candidly read these papers able to doubt that complete removal of the placenta does almost invariably put an end to hæmorrhage in cases of placenta prævia. This case is strongly affirmative of this thesis. While any part of the placenta remained attached to any portion of the uterus, the hæmorrhage went on profusely; but so soon as complete separation was effected, it ceased immediately and absolutely. Now, this result was not due to any pressure of the child's body against the placental surface, for I noticed the arrest of hæmorrhage, and had the observation for the sake of greater accuracy confirmed by Dr Cappie, before the membranes were ruptured, so as to render such compression possible. The stoppage of hæmorrhage before the rupture of the membranes seems also to negative the idea that its arrest is the result of contraction of the uterine walls, for no great change of area in their surface could take place so long as the membranes were intact. This case also tends to prove that the position taken up by Dr Tyler Smith, viz., that the removal of the placenta is not at all so likely to act as a means of arresting hæmorrhage as its expulsion before the child by the uterine efforts, is at least very doubtful. In this case there was at the period when the afterbirth was removed, and, indeed, up to the moment when the membranes were ruptured, little or no uterine action, and yet the removal of the placenta effected immediate and complete arrest of the hæmorrhage. Although I was wont to be inclined decidedly to the opinion that uterine hæmorrhage was in such cases directly from the free uterine surface, and not indirectly, in the first place, through the portion of the

uterus to which the placenta was still adherent; and, in the second place, from the free placental surface, I confess that this case has read me a lesson, and made me incline considerably to the less obvious opinion, that the hæmorrhage is after all indirect. It is often urged that the irritation of the hand during the act of removal of the placenta so irritates the uterine fibres in the placental site that they occlude the vessels, and prevent further regurgitation of blood. But my difficulty in acceding to this suggestion is, If it be true, why does it require the whole placenta to be separated? Ought not the irritation of removing a portion of the placenta, or at least so much of it as can be affected by the change in circumference of the lower portion of the uterus during a pain, to be quite sufficient? If irritation is an effective hæmostatic, and if the free surface of the placenta is not itself the chief source of hæmorrhage, partial separation ought for a time at least to be equally effective with complete separation. But this is found not to be the case.

It may be argued by those that are more inclined to the method of delivery by version in such cases, and who have rather faltering faith in the efficacy of complete removal, that we did wrong in removing the afterbirth, and that we ought to have attempted to turn. Well, we carefully weighed this consideration, and both Dr Cappie and myself were of opinion that there was next to no chance of effecting delivery by version so as to save the life of the child—the os was so imperfectly dilated, and the placental mass so obstructed the brim of the pelvis; besides, there was grave reason to doubt whether the child was as yet viable; and, moreover, from careful auscultation, I had already ascertained that the child's heart, from being some days previously strong and audible, had become by this time exceedingly weak and almost inaudible. I hold that it ought always to be with the accoucheur a matter of the most serious thought, and ought to demand his most conscientious consideration, whether in undertaking any operation involving risk to

the life or certain death to the child, he is clearly guided by sound principles of action. Now, we fairly considered this position, and were agreed in thinking that the extremely problematical chance of saving the child's life, even if it were viable, was not to be entertained, since by avoiding turning we lessened undoubtedly the risk of the mother. The extremely happy issue to the latter fully, I venture to think, justified our action. It is not fair to say, that if there was room to introduce the hand and remove the placenta, there must have been room to turn; for it is not the mere effect of turning that is dangerous to the mother, but the forcible distension of the preternaturally lacerable cervix uteri.

Without, therefore, attempting to decide in regard to the venous or arterial origin of the bleeding, I think this case tends to prove that the bleeding in placenta prævia is with fully as great probability referable to the torn placental surface than to the part of the uterine surface laid bare by its removal; while it shows, moreover, that be this true or not, practically removal of the whole placenta is a powerful means of arresting hæmorrhage in such cases.

In reference to the treatment of the second case I have nothing to remark, except that, considering the large amount of the placenta which was separated, it is astonishing that more profuse hæmorrhage did not take place; and that, had it not been for the evident tendency of the placenta to come down in front of the head so as to threaten to be expelled before the child, and also had not the prolapse of the cord taken place, the case might safely have been allowed to terminate itself. But seeing there was this double danger to the life of the child, I consider myself fully justified in turning so as to save its life.

The usual doctrine of the causation of placenta prævia receives confirmation from these two cases. Both happened in women who had been repeatedly pregnant, and consequently it might be reasonably expected that in both cases the cavity of the uterus was large, and thus

allowed the ovum to roll down to the os before it became embedded in the decidua vera.*

Dr Keiller said it was quite true that, even when there are no uterine contractions, separation of the placenta is not followed by any bleeding. He remembered attending a case in which there had been repeated floodings, and on passing the hand up, with a view to ascertain where the placenta was situated, such gushes of blood came on, that he separated the afterbirth, and turned the child. And in this case the child was born without any uterine pain, and the womb contracted perfectly. In such cases we ought not to be afraid of early interference; indeed, the mistake was often made of delaying too long. He remembered being called to a twin case, and on his arrival, one child was already born, and the double placenta was lying between the mother's legs. The second child was coming down by spontaneous expulsion.

Dr Pattison said he had met with a case in which separation of the placenta checked hæmorrhage.

Sir James Simpson remarked that Dr Macdonald had not alluded to one kind of practice, namely, partial separation of the placenta. He remembered long ago being told by Dr Tait, that when in practice at Lauder, he had been hastily summoned to a patient, and when he arrived, he found the woman walking about with the placenta dangling between her legs, and the hæmorrhage had stopped; and this was what led Sir James first to think of separation of the placenta in cases of placenta

* Since writing the above I have been led to modify my views of the treatment of placenta prævia very considerably. In conformity with the practice advocated and employed I understand, by Dr J. M. Duncan and others, I believe that early interference by dilatation and version, before the patient is weakened by much loss of blood, is the proper treatment of such cases. The use of Barnes's dilators remove almost entirely the difficulty of getting the cervix dilated, which used to be so serious. I had to treat two cases of complete placenta prævia last summer (1871), and in both of these cases I dilated with Barnes's bags whenever the bleeding became serious, and before the supervention of a single pain, and then turned. In both cases the success of the operation was complete. The mothers both did well. The children were both born alive, though, in consequence of prematurity, they both died. No hæmorrhage follows the extraction of the fœtus in such cases, as simple irritation of the uterus through the abdomen secures complete and powerful contraction of the organ.

prævia. The first time he introduced the practice was in the case of a patient of Dr Hill's at Portobello. The placenta was partially through the os, and having pulled it out, the bleeding ceased. It was an error, perhaps, to separate the placenta and afterwards turn, except to save the child. The late Professor George Wilson used to fancy, that if the placenta were put into oxygenated water, the child might thus be kept alive. Sometimes the hæmorrhage might be stopped by simply separating a little more of the placenta than what had already been detached; for, as the bleeding came from the line of vessels between the placenta and the uterus which are on the stretch, the separation relieved the tension, and by this practice there is a greater chance of saving the child.

Dr Cairns was of opinion that Dr Macdonald should have treated the first case exactly in the same way as the second. The first reason given for not doing so, as to the viability of the child, he did not think a good one; and the second reason, namely, that the os was not sufficiently dilated, was not any more satisfactory. It had been stated that the os was about the size of a crown-piece; and if that was so, he was surprised that Dr Macdonald should have allowed the patient to go on bleeding for two days.

Sir James Simpson, in reply to Dr Cairns, said that, no doubt, the reasons why Dr Macdonald did not turn were, the difficulty in dilating the os, and the danger of tearing the cervix.

NEW FORM OF GALVANIC PESSARY.

By James Young, M.D.

January 26, 1870.

Dr James Young made the following remarks on a new form of galvanic pessary, which he had been in the habit of using for some time. He said that, with Sir James Simpson's galvanic pessary, he had experienced in many cases the greatest difficulty in introducing it into the cervix uteri, especially in unmarried patients, in consequence of the large bulbous head. He had caused Gardner

to make several galvanic pessaries, of the usual length and thickness of stem, but with a small button-sized head or bulb, so that the difficulty he and others had experienced in their introduction might be removed. Dr Young said that he had found the use of galvanic pessaries attended with the greatest benefit in the treatment of amenorrhœa, where the usual remedies had failed. While he had experienced difficulty in their use, where the bulb was too large, there must be great care exercised in not having it too small—as in one case of his the pessary slipped up into the cavity of the uterus, involving considerable difficulty in its removal. He recommended that the head of the pessary should be in diameter five-eighths of an inch, and in thickness about one-eighth of an inch.

Dr Keiller said that he had seen the case alluded to, and, upon examination, he felt the twist of the pessary through the back part of the cervix, and on dilating, he felt the point of the stem. After chloroform was administered, he pressed the bulb of the instrument downwards through the cervix.

CHLORAL IN A CASE OF PUERPERAL MANIA.

By L. R. Thomson, M.D., Dalkeith.

February 9, 1870.

I have a few jottings from which I shall endeavour to speak as to the use of chloral in a case of puerperal mania lately under my care. Mrs S——, aged 29, was delivered of her fifth child on the 4th December last. The labour was physically easy and natural. The first symptoms of mental disorder were noticed soon after conception—became more decided at the usual period of quickening—and continued much the same until a few days before labour—during and after which great wakefulness and restlessness took place. It has been noticed, that when this disease appears during pregnancy, a more severe form

is to be apprehended after delivery. It was so in this case. From the time labour commenced, symptoms of acute mania appeared. She soon after became violent, and it was difficult sometimes to keep her in bed. To relieve this condition, and especially the insomnia, large and frequent doses had been given of morphia, sedative liquor of opium, and bromide of potass,—but all in vain. It was in these circumstances, and at the suggestion of Sir James Simpson, that I tried chloral. For five nights and days I had been unable to procure sleep. While in this state of excitement, I gave her a dose containing only 30 grains, and had the extreme satisfaction of seeing her asleep in five minutes. She slept for eight hours; when she awoke she partook of food, and again slept quietly for four hours. She was now much less violent, but after some hours, restlessness returned. A second dose of 30 grains was given, when sleep was again procured, but this time only for one hour. Two or three doses of 30 grains were subsequently given, but without the same decided effect as at first. I now gave 60-grain doses, and had the satisfaction of again procuring either sleep or quiet rest. On one occasion the patient became very cold after the larger dose, but there was no bad result. Sleep and food are the two great restorers in this disease. I was here indebted to chloral not only in procuring the former, but also in making it much easier to give the latter. Had sleep been procured earlier after the acute symptoms set in, the subsequent history of the case might have been different. From the time chloral was used, the disease became much less acute.

The patient has now been removed to an asylum. As there is no hereditary tendency to insanity, one is entitled to hope for recovery, even though it may be much postponed. Baillie used to say, in a case of this kind, "that the question was not whether she was to get well, but *when* she was to get well." Two forms of puerperal insanity are generally recognised—mania, more or less acute, and melancholia; the former occurring chiefly during and soon after

delivery, and the latter when the system is suffering from over-lactation. In reference to prognosis in such cases, Gooch has well said, that "mania is more dangerous to life, melancholia to reason."

From my observations in this case, I look forward to chloral being of the greatest service in the acute stage of puerperal mania.

HYDRATE OF CHLORAL IN PUERPERAL CONVULSIONS.

By Alexander Milne, M.D.

February 9, 1870.

On the 25th of January last, I was in attendance on Mrs ——, a healthy, but somewhat sensitive, woman, in labour with her fourth child. The case proceeded naturally and quickly, and the head was on the perineum, when a loud crash was heard in the house, caused by the falling of a ponderous lamp on the floor of the lobby. The suddenness and intensity of the sound, and also its unexpectedness, alarmed her exceedingly, and before she could be calmed down a violent convulsion followed, which lasted several minutes. The expulsive pains continued, and the child was speedily born. The placenta was also soon removed. Notwithstanding this, the fits continued with very short intervals, and with considerable severity. I administered gr. lx of the hydrate of chloral. The convulsions continued, though with less force and frequency, for about fifty minutes, when the patient fell into a heavy sleep. At first the breathing was sonorous, but gradually it became free, and the repose tranquil. The somnolent state lasted eight hours. There was no renewal of the fits. On awaking, the patient seemed rather confused, responding slowly to any question put; and it was with the utmost difficulty she could recollect the circumstance of the crash. She was free of headache or sickness. I

may state that, on being slightly pinched while asleep, she winced unmistakeably. She recovered well.

REMARKS.—I report this case as it is the first time I have employed Liebreich's much-vaunted drug in puerperal convulsions—a remedy more hardly ridden at the present time than any other medicine in existence. The case was a purely psychical one, due to the shock of a sudden, loud, and unexpected sound, and therefore well fitted to test the powers of the medicine, these cases being deemed rather obstinate. On the other hand, there was no albuminuria, and the absence of this complication lessened in some degree the seriousness of the case, and increased its amenableness to remedies. The benefit I obtained from the chloral was considerable, although not sufficient, in my estimation, to send one into ecstacies, or impress one with the belief that a novel cure had been brought before us of unexampled power. Sleep was not secured for nearly an hour, and yet a pretty large dose was given. In a suitable case—in a non-plethoric person, say—I think that, with hydrochlorate of morphia, I could have induced repose sooner, and perhaps even banished the convulsions as well. There might, it is true, have been more of after evil effects (that is, with morphia) than in this case; but in other instances I know headache, muscular exhaustion, and vomiting have been observed. Then, with chloroform, a drug still unrivalled in many respects, equal power and benefit at least have been conferred in puerperal eclampsia. I may say that I hold with Demarquay and others that chloral is not an anæsthetic, or rather, perhaps, that it is not a practicable and safe one. The patient winces and suffers while under a very large dose of it, and I suspect, if you push it on to insensibility, you are beyond the line of safety, and, in fact, on the confines of death. It will be no rival of Simpson's potent elixir therefore, but may contend strongly with some of the narcotics, such as morphia. As a hypnotic, many have found it of unspeakable service, and Bardeleben of Berlin thinks that it will abolish opium altogether.

Perhaps the eminent German is rather sanguine, and underrates the task of superseding a drug of such acknowledged power over acute suffering, and efficacy in many other ways. Time, however, will test and tell.

Sir James Simpson said that there was one circumstance which ought to be known in connection with chloral—viz., that there are two kinds of the drug in the market, a cheap sort and a dearer sort, and only the best should be trusted to. He had taken it himself as a hypnotic when suffering from a feverish attack. In order to know how long it took to produce its effect, he took a book with him to bed, and he only got as far as the second page when he fell soundly asleep, and slept so profoundly that he did not wake when called at the usual time in the morning. He experienced no bad effects from it. There is one drawback to the use of the drug, however—that is, its tendency to produce nausea in some people. He had one patient who took it regularly for chorea, and she was in the habit of taking it with lemon-juice, and found that in this way no sickness was induced. A distinguished clergyman in town, who had long suffered from sleeplessness, now takes chloral, and sleeps for nine or ten hours. Sometimes the eyes become inflamed after using it, but that soon passes away. He had been told by Dr Balfour that he had used chloral with success in delirium tremens. Sir James thought that Dr Milne's was the first case in which the remedy had been tried in puerperal convulsions. He had himself tried it in labour, first on Christmas day, in a long tedious case. One drachm was given, and then another; and though the patient slept an hour or two, the pains went on. In another case in which it was tried, the patient spoke away all the time, but afterwards declared that she had no recollection of the labour. Chloral has been used abroad by subcutaneous injection, and in a case in which this was done in the Infirmary it seemed to produce sloughing. It may likewise be administered by suppositories or pessaries. It is possible that by giving chloral smaller doses of chloroform might be needed. After the patient has fallen asleep care should be taken not to wake him up suddenly, as delirium is apt to ensue. Insufficient doses will sometimes cause delirium. Druggists prefer to give it made up in the syrup of lemon or orange. Chloral is not

constipating. He thought that Dr Young's observations as to the influence of the drug in epilepsy were most important.

Dr Cochrane thought that, in a case like Dr Thomson's, it might have been advisable to keep the patient constantly under the influence of the drug.

Dr Young said he had used the drug with success in infantile convulsions, etc.

Dr Ziegler gave it to a child as a hypnotic, first an eight-grain dose, which had no effect; next night ten grains also failed, while ten drops of morphia sent her to sleep. He had given it also to a lady suffering from gastralgia, and in her case forty grains produced profound sleep, and left no bad effects further than tenderness of the eyes, which passed off.

Dr Keiller had a patient who suffered from intense neuralgia of face and head, for which she had taken Battley's solution, which did not agree. He tried chloral in twenty-grain doses, and after the second the patient fell into a sound sleep, which lasted six or eight hours. In the morning the eyelids were inflamed. Next night half a drachm was given, but that did not do so well. One great advantage of the drug was its power of inducing sleep rapidly, and this was of importance in many cases. He remembered a case of puerperal mania which passed off in three days, and in that patient chloroform was administered, and morphia given by the stomach-pump.

Dr Mossop said he had used chloral in some cases in the Children's Hospital. One child was brought in with a severe scald, and screamed incessantly. Three grains of the drug were given, and in five minutes the child fell asleep, and slept all night. The next dose had not such a good effect, and the third did no good at all. In a case of meningitis, chloral was given in ten-grain doses with some benefit at first, but it lost its effect. It was given dissolved in glycerine.

THE ALLOA CASE OF DEATH UNDER CHLOROFORM.

By Sir James Y. Simpson, Bart., M.D.

February 9, 1870.

Sir James Simpson gave some particulars of the case of death under chloroform which recently happened at Alloa. He said it was a case of ovarian disease in which Dr Brotherston was going to operate, and he had been requested to be present. He gave the chloroform himself, but the first incision woke the patient up. More chloroform was given, and the cyst was tapped; but just as Dr Brotherston introduced his hand for the purpose of turning out the tumour, the woman suddenly vomited, the pupils dilated, a faint came on, the pulse could not be felt, and the patient died. The question was, Whether she might not have died from a faint, even suppose chloroform had not been given? Many cases had occurred of patients dying suddenly under operations when no anæsthetic had been employed. He knew of four cases in Edinburgh where death had resulted from sudden fainting without any chloroform being administered at all; and when it was known that 8000 doses of chloroform were manufactured every day in this city by one firm alone, it was surprising how very few accidents occurred from its use.

Dr Bell suggested the sickness and faintness might have been caused by the introduction of the hand into the abdominal cavity.

SPECIMEN OF A LARGE PLACENTA.

Exhibited by A. M. Rattray, M.D., Portobello.

February 23, 1870.

Dr Rattray exhibited a large placenta. The confinement had been natural, and the patient's age was 36. Her third child weighed 12 lbs. The placenta was expelled in twenty minutes, and the cord measured 29 inches.

CASE OF PUERPERAL TETANUS, COMING ON NINE DAYS AFTER PARTURITION, AND TERMINATING FATALLY IN FORTY HOURS.

By W. Craig, M.B., etc.

February 23, 1870.

In the *Edinburgh Monthly Journal of Medical Science* for February 1854, Sir James Y. Simpson drew the attention of the profession to the fact that tetanus, though a rare occurrence in midwifery practice, was nevertheless a much more common malady than was generally supposed. In that article Sir James makes this remarkable statement:—"In none of our modern obstetrical works, nor in any of the various essays or works devoted to the consideration of abortion or puerperal diseases, is any allusion made, as far as I know, to the possible supervention of tetanus after miscarriage." After making the above statement, Sir James shows that it may occur after injury of the unimpregnated uterus, after abortion, and after labour at the full time. He there records 27 cases of tetanus occurring in women after delivery, or after injury to the uterus, but of these, only about 20 occurred in Britain; and when we bear in mind that this malady is altogether unnoticed in obstetrical works, we can easily believe that "this fearful complication is fortunately a rare occurrence in midwifery practice." Since the publication of Sir James Y. Simpson's paper, in 1854, several cases of puerperal tetanus have occurred in this country, and have been duly recorded. It is now my painful duty to record a fatal case of puerperal tetanus, which happened in my own practice in November last.

Mrs S., aged 37 years, native of Ireland. Had previously seven children, all of whom were born in Australia. Her "labours" were always speedy, and her recoveries good. She mentioned to me that on her seventh confinement she had retention of the placenta, but, notwithstanding, she made a good and speedy recovery.

The veins of her right leg were varicose, and on several occasions before her confinements one of these veins (a little above the internal malleolus) burst and caused considerable hæmorrhage. This had occurred before several of her confinements in Australia.

On the 4th of September last, I was called in to arrest the hæmorrhage which had occurred from this old wound. This I accomplished easily, by means of a pad and bandage. On the 30th of September I was again sent for, to arrest the hæmorrhage from this same wound.

The next time I saw the patient was on the evening of her confinement.

On the evening of Wednesday, 27th October, shortly before midnight, I was requested to come immediately to see her, and in five minutes afterwards I was at her bedside.

On entering her bedroom, I learned that several hours previously she had been delivered of a strong healthy boy, a woman having acted as accoucheur. The placenta was still retained, although the nurse had made several unsuccessful attempts to remove it. This midwife was very averse to a medical man being called in, and only consented after repeated attempts to remove the placenta had failed.

I found the patient very weak, chiefly from hæmorrhage. The uterus was flabby and uncontracted. Having caused the uterus to contract by external manipulation over the abdomen, I very soon was enabled to remove the placenta, being careful at the same time to remove all clots from the vagina. I found, however, a large amount of coagulated blood in the bed beside the patient.

By the time the placenta was removed the patient was very faint, and I accordingly administered a little stimulant, and she soon revived. The patient passed a good night, and told me next day that she had slept well, and never was better after any previous confinement. For more than a week she appeared to be making a fair recovery. The secretion of milk was very copious; and

on Thursday, 4th November, eight days after her confinement, the patient was so well that she insisted on getting up out of bed, but as her pulse was fully 80 per minute I refused to allow her. Her pulse since her confinement had generally kept above 80, and sometimes was as high as 100 per minute.

The high pulse was suggestive of mischief; but as her appetite was good, and the secretion of milk abundant, I hoped that no bad results would follow.

On the morning of Friday, 5th November, whilst she was at breakfast, she suddenly felt something wrong with her throat when swallowing some tea. She told me of this when I saw her in the course of the forenoon, but I thought she had caught cold, and told her to wrap her neck in flannel, and at the same time gave her ten grains of the compound powder of ipecacuanha.

On the afternoon of the same day, I was sent for, as she was worse; but being professionally engaged elsewhere at the time, it was several hours before I could see the patient. Meanwhile, another medical man was called in to see her, who ordered a mustard poultice to be applied to the throat. I saw her in the course of the evening, and found that the mustard ordered by my brother practitioner had produced no benefit. I found, moreover, that now she had great pain and difficulty both in opening her mouth and in swallowing. Her own idea was that the "pap of her throat" (uvula) had fallen down, and that if I could only lift it up she would be all right. Of course I knew she was mistaken; but I mention the fact to describe the patient's own feelings at the time. She could not open her mouth sufficiently to allow me to see as far back as the uvula, and she had very great difficulty in swallowing. On external manipulation, however, no pain was elicited, showing the absence of inflammatory action; in fact, she experienced pain only when trying to open her mouth or when swallowing.

Although at this time there was a certain amount of trismus, I could not bring my mind to the conclusion

that it was a case of tetanus. Her bowels being somewhat constipated, I ordered her some opening medicine. Hoping it was only a spasm of the muscles produced by cold, I ordered hot poultices of bran and chamomile flowers, along with hot fomentations of the same. All these were applied most faithfully and effectually during the whole night.

Next morning (Saturday, 6th), between 6 and 7 o'clock A.M., I was sent for, as she was no better but worse, the poultices and hot fomentations, although constantly applied all night, having produced no change for the better.

I may here mention, that the patient was fully convinced that something was sticking in her throat, and she wished me to cut in and see what was wrong and rectify matters. Such was her feeling.

It was agreed by the husband and myself to call in another doctor in consultation. I according went to Dr Patrick Heron Watson, who kindly came and saw the patient with me. She could still swallow, but with very great difficulty, and Dr Watson was unable to see the back part of her mouth, owing to the rigidity of the masseter and temporal muscles. The pulse was about 130 or 140 per minute.

After a careful examination of the patient, Dr Watson gave it as his opinion that it was a true case of tetanus. At his suggestion I ordered tincture of Indian hemp, 25 minims to begin with, and 10 minims every hour afterwards; also to continue the poultices and hot fomentations. The medicine I gave the previous evening not having acted sufficiently, I ordered a turpentine enema to be given. This had the desired effect of thoroughly clearing out the bowels. The fæces had a most offensive smell.

Notwithstanding all our treatment, the patient got rapidly worse; and after she was unable to swallow, I ordered nutrient injections per rectum, and even gave the tincture of Indian hemp in the same way.

About mid-day on Saturday, in addition to well-marked trismus, there was added opisthotonos, the longitudinal

muscles at the back of the neck being the muscles chiefly affected. They were in a state of tonic spasm. I ordered the hot fomentations and poultices to be applied to the back of the neck also. All our treatment, however, proved unavailing, for the disease, which was now well marked, made rapid progress. The patient was perfectly conscious, and described her feeling as if a "ton weight was dragging her head backwards." What added greatly to the patient's sufferings was an accumulation of mucus in the throat which could not be got rid of, and which produced a sense of constant suffocation and impending death.

I will not soon forget her pitiable expression and her truly pathetic appeal to me to give her something to mend her throat. It is indeed a sad sight to witness a patient in the midst of terrible agony and impending suffocation making an earnest and *confident* appeal to her medical attendant for relief, when the physician knows too well that "vain is the help of man" in such a case.

At this stage of the disease I witnessed an interesting but painful phenomenon. Every five or ten minutes her head was observed to jerk a little backwards, then remain stationary for other five or ten minutes, and then suddenly to jerk again backwards. This interesting phenomenon was well marked, and continued as long as I saw the patient. It had all the appearance of a screw gradually but surely drawing the occiput backwards towards the shoulders. Jerk after jerk reminded one of another turn of the screw. In short, opisthotonos was now well marked. The expression of the countenance was characteristic of the disease. The teeth were quite exposed, the angles of the mouth drawn much backwards, giving a very ghastly appearance to the patient. The "risus sardonicus," so characteristic of this disease, was in this case very distinct.

I saw her for the last time about 1 A.M. on Sabbath morning, and in two hours afterwards death terminated her sufferings, the disease having lasted forty hours.

From the symptoms described to me, I believe the immediate cause of death was asphyxia.

There can be no doubt but this was a case of tetanus. In this opinion I am fully confirmed by Dr Patrick Heron Watson. The trismus, opisthotonos, exposure of the teeth, and ghastly grin (risus sardonicus), were all well marked. It was, moreover, a case of tetanus coming on nine days after parturition at the full time. Of this latter fact there can be no doubt. The deceased expected her confinement in the beginning of September, at least six weeks before it took place; and the child was much above the average in size—in fact, it was the largest new-born child I have ever seen.

There was no appearance of either peritonitis or metritis. The lochial discharge still continued to flow. It threatened to stop, but I ordered hot fomentations, and by this means it readily continued discharging. The secretion of milk continued till the day of her death. Let it also be borne in mind that it happened in Edinburgh during the cold weather of November. The patient, however, was only a few months returned from Geelong, Australia, where the greater part of her married life had been spent, and where her other seven children were all born. It is necessary to bear these things in mind, since tetanus is a disease much more frequent in warm countries than in cold ones; but I am far from asserting that the fact of her having spent many years in the warm climate of Geelong had anything to do with bringing on tetanus in this case.

The connection between tetanus and parturition is still involved in much obscurity. Whether or not tetanus could have been avoided had I been present at the delivery of the deceased, I am not prepared to say, although in that case I would most certainly have removed the placenta much sooner, and would have thereby saved the woman much loss of blood; or whether, had a more experienced accoucheur been called in instead of me, the result would have been otherwise, I

am equally unable to state; for this must always be borne in mind, that the frequency of traumatic tetanus, of which puerperal tetanus must be regarded as a variety, bears no proportion to the extent of the injury.

Puerperal tetanus is, fortunately, in this country at least, one of the rarest complications in midwifery practice, and I believe is relatively less frequent after parturition at the full time. This is just what one would expect, for in the case of abortion nature has not completed her preparations for the expulsion of the ovum. In surgical practice, tetanus is supposed to occur most frequently after lacerated wounds; and reasoning *a priori*, it is in cases of abortion where we would expect tetanus in obstetrical practice.

In regard to the causes of tetanus we know very little. Cold is often said to produce idiopathic tetanus, but how a cold comes to produce this fearful malady no man knows. We all know that tetanus sometimes follows a wound, but the connection between the wound and that morbid state called tetanus is still an unsolved problem. We are equally ignorant regarding the causes of puerperal tetanus. In some of the cases hæmorrhage has been assigned as the cause, and in the case I have described there was much hæmorrhage, and possibly also cold; but it is very difficult to say whether these were causes or only coincidences. Certain it is that tetanus is by no means a common result of hæmorrhage. Similar observations might be made regarding most of the assigned causes of puerperal tetanus. No matter how difficult the task, we must always endeavour to distinguish between *post hoc* and *propter hoc*.

Facts are still wanting to prove the influence of temperature on tetanus in this country. The disease is much more frequent in certain warm climates than in Britain, but it has yet to be proved that in this country it is more frequent in summer than in winter. Of twenty-seven cases recorded by Sir James Y. Simpson, only one is mentioned as occurring in June, whilst several are men-

tioned as occurring in the depth of winter; and the case under my care happened in the cold frosty weather of November last.

There is one fact well ascertained regarding puerperal tetanus, namely, that it is exceedingly fatal. Of the twenty-seven cases recorded by Sir James Y. Simpson, twenty-two died and five recovered. Of the five recoveries three were Continental cases, two of which at least were ascribed to cold, and thus resembled to a certain extent idiopathic tetanus, which is not nearly so fatal as the traumatic form of the disease. The other two recoveries were in England; but it has been doubted, and not without good reason, if these were true cases of tetanus. It would thus appear, that up to 1854 there was no well-authenticated case of recovery from puerperal tetanus in Britain. Whether or not there has been any case of recovery since, I have no means of ascertaining, but this I know, there have been several fatal cases since that time.

Much needs yet to be investigated regarding the pathology of tetanus. The probability is that it is essentially a disease of the nervous system, the principal seat of which is the spinal cord. It has been ascertained that the bloodvessels of the cord are often congested, and that extensive lesion exists in the substance of the cord. The gray matter is sometimes found softened and disintegrated. But the strange fact still remains, that similar lesions of the spinal cord have been observed in other diseases where there were none of the spasms of tetanus. It will be very interesting to learn whether or not any lesions exist in the nerve or nerves leading from the seat of injury to the gray matter of the cord, and what is the nature of these lesions, if any exist. It will be also important to know the nature of the influence transmitted from the periphery to the centre. Till once we know exactly what is the morbid anatomy of this disease, we can never arrive at its true etiology.

In regard to the treatment of this disease, I have not much to say. I believe our only chance of success lies

in an early diagnosis of the disease, and so cutting it short by what may be called an "abortive treatment." We must endeavour to check the malady before organic lesion takes place; for, after extensive lesion and disintegration of the gray matter of the spinal cord, I believe no remedy will be of the slightest avail. Unless we can check the disease when it is chiefly functional, there is little hope for our patient. Few indeed are the cases where medicines rectify great organic lesions.

Dr Keiller said that Dr Craig's paper was one of great importance, and that he could bear out the remarks made by him on puerperal tetanus. Some years ago he had himself a case of puerperal tetanus during abortion, when the patient recovered. There had been plugging in that case, and some supposed that that was the cause of the spasms. There was the usual painful opening and closing of the jaws. Opium was used as the treatment. Dr K. suggested the use of chloral. The German discoverer of the drug says that chloral prevents the convulsive effects of strychnine. Sir James Simpson mentions a case of fatal tetanus following the extraction of a polypus.

Dr Cochrane entirely coincided with Dr Keiller's views on puerperal tetanus, but thought that the use of opiates by injection would prove more useful than chloral.

Dr Cairns asked whether the patient had vomited, or any appearance of a rash had been detected, to which Dr Craig replied in the negative. Dr Cairns said that he agreed with all that had been stated as to the extreme rarity of this disease. He had consulted numerous works, but could find no notice taken of puerperal tetanus, and he confessed that he had come to the meeting with the intention of disputing whether this was really a case of tetanus at all. But after hearing Dr Keiller's remarks, he was bound to admit that the affection was actually tetanic. As to the suggestion that chloral might have been of benefit in such a case, he thought that it might have been more likely to produce harm. He had tried it on his own patients and on himself, with the effect of producing serious consequences.

Dr Bell called attention to the peculiar character of the spasm, and suggested that the woman might possibly

have taken strychnine. He also thought that chloral might have been of use.

Dr Menzies mentioned that Dr Burn had met with a fatal case of puerperal tetanus after abortion some years ago.

Dr Ritchie recollected a case of traumatic tetanus in the Children's Hospital, in which Indian hemp, to the amount of an ounce, had been administered with success.

THE STETHOSCOPE AS A MEANS OF DIAGNOSING THE SEX OF THE CHILD.

By James Cumming, M.D.

February 23, 1870.

AUSCULTATION OF THE FŒTAL HEART.

Table I.—Males.

	No. of Pulsations per Minute.	Character of Pulsations.		No. of Pulsations per Minute.	Character of Pulsations.
1.	*a.* Heart in right groin, 120. *b.* Heart high in left hypochondrium, 154	.....	12.	136	.....
			13.	133	.....
			14.	134	.....
			15.	116	.....
2.	138	.....	16.	120	Distinct, 116-124
3.	138	.....	17.	120	.....
4.	135	.....	18.	138	.....
5.	130	.....	19.	125	Distinct.
6.	130	.....	20.	140	Distinct.
7.	132	Distinct.	21.	140	Distinct.
8.	132	.....	22.	137	130-144
9.	140	.....	23.	140	.....
10.	132	.....	24.	141	.....
11.	140	Distinct, 136-142	25.	122	.....

Table II.—Females.

	No. of Pulsations per Minute.	Character of Pulsations.		No. of Pulsations per Minute.	Character of Pulsations.
1.	150	.....	9.	140	Indistinct.
2.	142	Indistinct, 140-144	10.	152	Indistinct.
3.	150	144-150 152-160	11.	140	Indistinct.
4.	140	.....	12.	143	140-146
5.	144	Indistinct.	13.	144	Indistinct.
6.	140	Indistinct.	14.	141	142-140
7.	140	Indistinct.	15.	160	.....
8.	144	.....			

TABLE III.—EXCEPTIONS.

	Pulsations per Minute.	Character of Pulsations.	Sex.
1.	136	.....	Female.
2.	134	.....	Female.
3.	138	.....	Female.
4.	130	.....	Female.
5.	118	116-120	Female.
6.	136	.....	Female.
7.	128	Very Indistinct, 126-130	Female.
8.	120	Indistinct.	Male.
9.	132	.....	Female.
10.	136	Very Indistinct.	Female.
11.	124	120-128	Female.
12.	132	... ..	Female.
13.	122	120-124	Female.
14.	118	.....	Female.
15.	158	Distinct.	Male.
16.	120	.....	Female.
17.	150	.....	Male.
18.	116	112-116	Female.
19.	150	.....	Male.

REMARKS.—In Table I. the first case was one of twins, where the heart of the one fœtus was heard in the right groin, beating 120 in the minute (distinct), and on delivery, it proved to be a male. The second fœtal heart was heard in the left hypochondrium, beating 154 per minute, and on delivery, it proved to be a female.

In Table I., the pulsations varied from 120 to 140 per minute; and in the majority, the pulsations were characterised by distinctness. On applying the stethoscope to the abdomen, the fœtal heart was at once heard beating, and could be easily counted. On delivery they all proved to be males.

In Table II., the pulsations varied from 140 to 160 per minute, and were, as a rule, indistinct. It was often several minutes before the heart could be made out, and difficult to count the pulsations from their rapidity and indistinctness.

Table III. consists of exceptions to Tables I. and II. —that is to say, in fifteen cases when the pulsations varied from 116 to 138 per minute, the fœtuses were

found to be females; and in three cases, pulsations 150-160, they proved to be males. I include one case in this table, where, although the pulsations were 120 per minute, they were so very indistinct that a female was predicted.

There are two points thus to be observed when auscultating the fœtal heart:—

1. The number of pulsations per minute.
2. The character of the pulsation, whether distinct and readily diagnosed or indistinct.

Judging from Table I., where the pulsations vary from 116 to 140 per minute, and are distinct, a male may be predicted.

And again, judging from Table II., where the pulsations vary from 140 to 160, and are indistinct, a female may be predicted.

But Table III. contains all negative results, as in fifteen, with pulsations varying from 116-138 = females; and in three, with pulsations 150-160 = males.

Of fifty-nine cases, the diagnosis of forty was correct, and nineteen incorrect.

From the above few cases, it would be premature to form any conclusions; besides, there are several other points which should, if possible, be ascertained, namely:—

1. The ratio of the maternal pulse to the fœtal pulse.
2. The ratio of the paternal pulse.
3. The weight of the child.
4. The quantity of liquor amnii, etc.

Dr Keiller remembered that this subject had been investigated a long time ago by Dr John Buchanan, but no reliable results were arrived at.

Dr Bruce thought that Dr Cumming's observations tended to prove that we cannot with certainty ascertain the sex, but was of opinion that the subject should be further examined.

EPITHELIAL CANCEROUS GROWTH.

By Alexander Keiller, M.D.

March 9, 1870.

Dr Keiller showed an epithelial cancerous growth, which he had removed from the posterior lip of the cervix uteri of a patient who had been sent to him by Dr Young of Kirkcaldy. It is a good specimen of what is called the cauliflower excrescence; and the woman for some time back had lost a large quantity of blood. The operation was performed two days ago by means of the écraseur, and under the influence of chloroform. The chain was passed behind the cervix, and the structures were slowly divided, the division being completed by scissors. There was very little bleeding, but the solution of perchloride of iron and glycerine was applied. He was in hopes that the cervix had been divided sufficiently high up to prevent the return of the disease.

ADDITIONAL CASES TREATED AT THE ROYAL MATERNITY HOSPITAL.

By Charles Bell, M.D.

March 9, 1870.

The following cases seem to me to be sufficiently interesting to entitle them to be brought before the Society, and I hope they may be the means of inducing such discussion as will extend the general knowledge of the different subjects which they tend to illustrate.

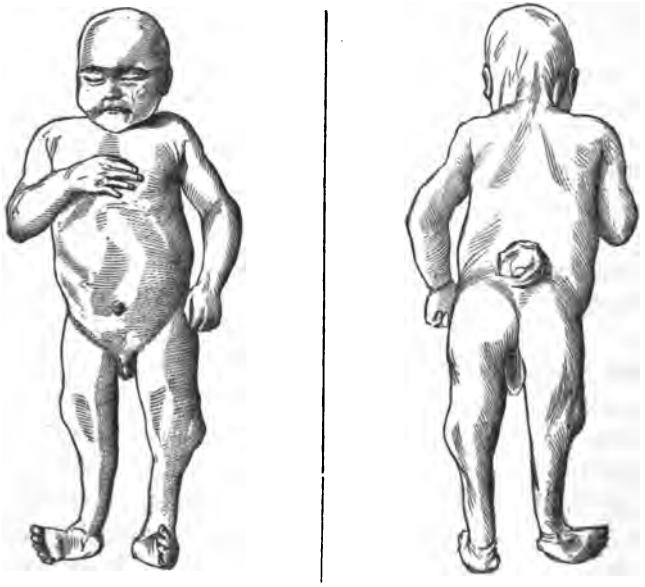
Two Cases of Spina Bifida.

Mrs Ashly, Grassmarket, aged 31, was delivered on the 29th January 1869 of her eighth child. She stated that she was last unwell in the month of June, and quickened on the 1st September; she had had no miscarriages; that all her children had been born at the full time, and were well formed. Her husband goes about the country with Punch and Judy, and she has accompanied him for many years, carrying a heavy box on her back for hours daily, which has induced her to stoop very much.

Her labour was tedious, having commenced at 2 A.M. on the 28th, and it was not completed until 4 A.M. the following day. The fœtus presented in the first position, and the caput succedaneum was situated on the upper part of the right parietal bone. There was nothing particular in her labour except its tediousness.

The child had a combination of malformations. The penis was contracted at the middle of its length, and the glans was much swollen. The flexor muscles of the lower extremities were contracted so that the legs lay in a bent position; and the two patellæ were situated on the outside of the knees. The natural position of the limbs, therefore, when not extended, was very much like Punch's on the stage, the feet being in the state of talipes varus. The hips were flattened, and there was a large spina bifida situated on the lower part of the lumbar region and upper part of the sacrum, measuring 2½ inches in length and 1½ in breadth.

It took very little nourishment, and died in convulsions



on the fourth day after birth. The accompanying woodcut gives an excellent illustration of the appearance of

the child. It was taken from the cast exhibited to the Society, which was made by Dr Cumming, the house surgeon, who had the charge of the case.

Agnes Porteous was delivered of her first child on the 2d January 1870. Her labour presented nothing unusual; but the child had several malformations. There was a spina bifida, about the size of a hen's egg, on the lumbar region. The tumour was flaccid and transparent at birth; but on the following day it became distended with fluid, and on the third day opaque flakes made their appearance, and there was a sort of plastic lymph secreted, and the tumour threatened to suppurate. On the fifth day it burst, and its fluid flowed gradually off. A slough formed, which was removed. On the sixth day there was considerable discharge of fluid. The child became restless, and refused to take the breast. It all along kept up a low moaning sound, which was occasionally interrupted with sharp cries. Thrush made its appearance, accompanied with dark-green evacuations from the bowels, and reddish discharge from the vulva. The umbilical cord separated, leaving a foul sore.

Diluted carbolic acid was applied to the back, and chloral in doses of from three to five grains was given to procure rest. On the seventh day, convulsions came on, and chloral was again administered with apparent benefit, as it produced quiet; but the child gradually sank and died. In addition to spina bifida, there was talipes calcaneus of the left foot, and talipes varus of the right foot.

Dissection of the Tumour.—There was found a deficiency of the transverse and spinous processes of the third and fourth lumbar vertebræ. The sac was formed of the membranes covering the spinal cord; but in consequence of the state of the parts, the connexion of the cord itself with the tumour could not be distinctly made out.

REMARKS.—This kind of malformation has been described under different names by authors; such as hydro-rachis, hernia spinalis, and meningole of the spinal cord—a designation given to it by Foster. It is characterized by a

deficiency of one or more of the laminæ and spinous processes of the vertebræ, and it may occur at any part of the vertebral column; but it is met most frequently on the lowest lumbar vertebra, and the first of the sacrum, and the last vertebra of the neck. It varies in size from that of a pea to that of an orange generally; but sometimes it extends from the neck to the sacrum. It is transparent, having no covering from the skin; and it varies in colour, being sometimes red, at others it is livid like a cancerous tumour. It is soft in the centre, but where it joins the true skin it sometimes has a bony hardness like the caput succedaneum. It is usually filled with what is considered the cerebro-spinal fluid. Cruveilhier states that, "if the tumour corresponds to the upper lumbar vertebra only, the spinal cord very seldom deviates from its course, and the posterior spinal nerves are generally the only branches which have connexion with the sac;" but if it occupies partly the lumbar, and partly the sacral region, the cord itself, and its nerves, will be found intimately connected with the sac; hence the paralysis which sometimes accompanies the disease.

According to Dr Coley, this disease commences at the second month of gestation, when the process of ossification is liable to be arrested in certain parts; but at whatever period it begins, it seems to have no effect in general on the growth of the fœtus in utero, as it is in some instances healthy, often large and very strong; in other cases, however, the ankles are weak, distorted, and paralytic. For the most part it proves fatal in infancy, or rather a short time after birth; but there are some instances of cure, one of which is related by M. Gosselin, who states that Dr Sezerie injected the tumour in a child of nine months of age with perfect success. The child had a paraplegia, and both arms were paralyzed. The tumour was as large as an egg, and was situated on the lower part of the lumbar region. It burst twice spontaneously. Dr Sezerie used a trocar and canula, and, after letting off the fluid, he injected equal parts of tincture of iodine and water, which was retained some

minutes. After this operation the tumour did not return; the paralysis of the arm disappeared, and the paraplegia gradually diminished, and the child ran about when two years of age; the only remains of paralysis was incontinence of urine. *

Dr Tanner † states that the injurious effects of the disease vary accordingly to the locality of the tumour and its contents. When situated on the neck it generally proves fatal in a few days after birth; but it is less dangerous when it is situated on the lumbar and sacral regions. He refers to cases in which the patients lived to the age of twenty or thirty years, and some even attained the natural period of life. Chaussier, who had extensive opportunity of observing the disease in the Maternité in Paris, states that it occurs once in every thousand births.

Treatment.—As a general rule, it is considered that the less this disease is interfered with the better, as any kind of operation is attended with great danger. But if the tumour is increasing in size, it is justifiable to try something, and the most obvious resource is to puncture it and then inject iodine, afterwards to apply gentle pressure. In adopting this treatment, Hewitt considers that great caution must be used, lest in puncturing the tumour the spinal cord or nerves should be injured.‡ The puncture should therefore be made at the side and lowest part of the sac. The child should have nourishing diet if weaned, and a strong healthy nurse if still nursing, along with pure country air.

Case of Excess of Extremities.

Mrs M'Guire, Grassmarket, was delivered of her thirteenth child, a girl, on the 4th February 1869, after an easy labour, which was so rapid that the child was born some time before the medical attendant arrived; but when he came he found the placenta still retained. On

* British and Foreign Review, No. xxix., p. 208.

† Diseases of Infancy and Childhood, p. 125.

‡ Tanner, *op. cit.*, p. 126.]

examining the cord he found that there was a firm knot about the middle of its length, which he untied. She was not at her full time, having altered on the 10th May, and the child gave unquestionable signs of being premature, as its upper extremities were only three and a-half inches in length, the arm being one and a-half inch, and the forearm two inches—a circumstance which indicates that the child was little beyond the seventh month or commencement of the eight month; which Casper * considers "the most important period in embryonic life in a medico-legal point of view, because at the end of the thirtieth week, or 210th day, the fœtus indubitably, and, according to statutory declaration, commences to be viable." The child had six fingers on each hand, all of which had nails. There was talipes varus of both feet, and the anus was occluded. There were several marks on the head as if the result of blows.

The patient stated that five of her children were born in the eighth month, and died soon after birth. One of these had twelve fingers and twelve toes—a peculiarity which seemed to be hereditary, as both father and grandfather had had the same. She accounted for the appearances on the child's head by her having been knocked down and kicked on the abdomen by a drunk man on the 1st January.

The child took some food and its bowels were moved, but the evacuation passed through the vagina. It died the following day at 9 A.M.

REMARKS.—Excess in the extremities, especially in regard to the number of the fingers and toes, is not an uncommon deformity; and it is not unusual to find it hereditary, as it was in this case. The supernumerary extremities are generally smaller than the natural fingers and toes. They are always situated either on the inner or outer border of the hands and feet, and as they have no muscles, they are useless appendages. It is therefore desirable to remove them soon; but in doing so it is

* Forensic Medicine, vol. iii. p. 16.

necessary to guard against injuring the joints of the natural extremities, as sometimes they are implicated with the supplementary organs, It is also to be remembered that erysipelas is extremely apt to occur after the removal of these supplementary fingers and toes. This took place in a patient of mine which was operated upon by Mr Syme. The erysipelas soon yielded, however, to the use of the muriated tincture of iron, and the child made a good recovery. This child had six fingers on each hand, and the little toe of her right foot was webbed —a peculiarity which several of the family had. The father of this family had six toes on each foot, and all his brothers, except one, had either supernumerary fingers or toes. There is a general impression in certain places that the excess of extremities is a sign of good luck—a circumstance which was fully exemplified in the family here referred to, as every member of it was most successful in a worldly point of view.

Dr Keiller said that he had been called to the country yesterday to see a case of spina bifida. The child was three weeks old, and the tumour, which was uncovered by skin, was situated in the lumbar region. It felt soft but did not fluctuate. Carbolic acid plaster had been applied. His opinion was sought as to whether any operative interference should be resorted to, but he did not think that any operation would be of benefit. He remembered once seeing a spina bifida in a girl of twelve, who was able to go about; and cases had been reported in which the patients had lived thirty or forty years. The higher the tumour was situated, the more fatal it proved.

Dr Menzies said that he believed that Dr Henderson of Leith had had a case of spina bifida which was successfully treated by operation. The mother of the children whose cases he (Dr M.) had reported to the Society was again pregnant, and for some time he had been giving her the chloride of calcium, in the hope that in this way the tendency to spina bifida might be averted. Dr Menzies, replying to Dr Craig, said that there was no family history to account for the peculiarity, and the four children born previously were quite healthy.

Dr Milne said that, in his experience, the occurrence of supernumerary fingers and toes was the most frequent kind of malformation.

Dr Keiller said he remembered seeing a family in Fife where seven or eight of the children had some of the toes absent, and their places supplied by little rounded berry-like appendages.

Dr Auguste Corales said that his father had two fingers exactly the same length, and so had he.

PREGNANCY WITHOUT MENSTRUATION.

By James Young, M.D.

March 9, 1870.

Dr James Young read to the Society some statistics which he had collected, showing how frequently pregnancy had occurred where the women had never menstruated more than once or twice during ten or twelve years, and where six or eight children had been born. Among other cases, the following two might be specially mentioned:—

Case I.—Mrs M—— was married on 10th September 1859; menstruated in October thereafter, but not again to this date (June 1870), and she has had six healthy living children.

Case II.—Mrs J—— was married in January 1856, and has only menstruated three times up to this date (June 1870), and is now the mother of nine children, seven of whom are alive.

Dr Young remarked, that in both cases the patients had menstruated regularly previous to their marriage.

Mr Pridie said he had attended a girl in her first confinement, who was 15 years of age, who had never menstruated; and he knew of a lady who had been married for twelve years, had seven children, and had only been seven or eight times unwell.

Dr Keiller said that the general belief was, that ovulation

might take place without menstruation. Cases like those described by Dr Young were very rare. He had often heard of cases in which women were said to have menstruated all through their pregnancy, but had never met with one himself.

Dr Craig said that in Mr Pridie's case the menstrual periods most likely corresponded to the times at which the lady fell in the family-way.

CASE OF RETARDED LABOUR.

By Francis D. M'Cowan, M.D.

March 9, 1870.

On the morning of the 5th January, between the hours of three and four o'clock, I was called to see M. M., who was reported to be in labour. It was her first confinement. I found upon inquiry that labour had commenced on the previous evening at about eight o'clock, the pains recurring only at intervals of fifteen minutes. I did not consider it necessary to remain. I returned, however, about eight o'clock, and found the patient in much the same condition. On paying several successive visits up till Thursday evening, I found the pains occurring every five minutes, and proceeded to make an examination per vaginam, and found that the membranes had ruptured, and on inquiry learned that they had done so at the commencement of labour. The os was only dilated to the size of a shilling, although labour had been going on for twenty-four hours. I also found an elongated coccyx, which was quite flexible, and which, during a pain, formed a complete curve, due to the contraction of the coccygeal muscles. I continued to dilate the os, and, during a pain, to push back the coccyx, so as to facilitate labour; but I found that after five or six pains, labour entirely ceased for more than an hour, and again commenced and advanced the child somewhat, and again the pains ceased. The head being now pretty well down, and the os fully dilated, I administered to the patient ʒij Tinct. ergotæ,

which acted upon the uterus in twenty-five minutes; but, owing to the prominency of the coccyx and the action of the muscles, the child's head was prevented from passing through the outlet. Inertia of the uterus again occurred for a considerable time, and I allowed the patient to rest herself.

On the evening of Friday the 7th, I again administered ʒiij Tinct. ergotæ, and employed friction externally; labour recommenced in fifteen minutes. I, during a pain, pushed back the coccyx, when the child's head passed through the outlet. Another pain occurred, and the child was born at 8.30 P.M., but apparently dead. I adopted the method recommended by Dr Marshall Hall to restore animation, and after half an hour the child breathed and cried lustily. The umbilical cord was of considerable length, being three times round the child's neck; placenta large and healthy. The patient made a good recovery.

Two weeks after delivery, I examined the woman to ascertain as accurately as possible the form and dimensions of the coccyx. In form it was rounder at the edges and more triangular than the ordinary coccyx, and having a distinct neck, with a nodule at its apex of an oval shape. The length from base to apex was 2½ inches, from base to neck, 2¼ inches; at the base the transverse measurement was ¾ of an inch, at the neck, ¼; the long diameter of the nodule, which is placed transversely, amounts nearly to ½ an inch. The coccygeal muscles were very strongly developed.

Dr Cochrane was of opinion that forceps should have been used.

OBSTETRICAL INSTRUMENTS USED IN FRANCE.

Exhibited by Dr Cordes of Paris.

March 23, 1870.

Dr Cordes of Paris exhibited the following obstetrical instruments used in France:—1*st*, Forceps, invented by

Chassagny of Lyons; 2*d*, a Dilator for the Cervix Uteri; 3*d*, Pajot's Crotchet; 4*th*, a Perforator; and 5*th*, a Cephalotribe. Dr Cordes made some remarks in explanation of the various instruments shown, and received from the President the thanks of the meeting.

REMARKS ON PELVIC PERITONITIS AND PELVIC CELLULITIS, WITH ILLUSTRATIVE CASES.

By LAUCHLAN AITKEN, M.D.

MARCH 23, 1870.

RATHER more than a year ago there appeared from the pen of a well-known gynækologist of this city a very able monograph on the two forms of pelvic inflammation whose names head this article; and it cannot have escaped the recollection of the reader that Dr M. Duncan, adopting the nomenclature first proposed by Virchow, has used different terms on his title-page * than those older appellations I still propose to retain. Under these circumstances I feel compelled at least to attempt to justify my preference for the original names; and I trust to be able to show that they are preferable to, and less confusing than, any others that have as yet been proposed, even though we cannot consider them absolutely perfect.

Passing over, then, such terms as *periuterine cellulitis* or *phlegmons périutérins* as bad compounds; others, as *inflammation of the broad ligaments*, as too limited in meaning; and others, again, as *engorgement périutérin*, as only indicating one of the stages of the affection,—I shall endeavour as succinctly as possible to state my reasons for preferring the older names to those proposed by Virchow.

1*st*. The two Greek prepositions, *peri* and *para*, are employed somewhat arbitrarily to indicate inflammatory processes which are essentially distinct. I say arbitrarily,

* A Practical Treatise on Perimetritis and Parametritis (Edin. 1869).

because I am not aware that *para* has been generally employed in the form of a compound to express inflammation of the cellular tissue elsewhere.* By those who remember that the cellular tissue not only separates the serous membrane from the uterus at that part where the cervix and body of the organ meet, but is even abundant there,† perimetritis might readily be taken to indicate one of the varieties, though indeed not a very common one, of pelvic cellulitis—a variety, in fact, for which the term perimetric cellulitis has been proposed. It is self-evident how much confusion might arise as to the real nature of a disease simply by the change of one letter of the alphabet.

2*d*. The term *perimetritis*, even if employed only to indicate inflammation in the peritoneum, could not well be used if that inflammation were confined to the serous membrane round the tubes or ovaries. For such varieties of pelvic peritonitis we should require to adopt the terms peri-salpingitis and peri-oophoritis; thus, in my opinion, unnecessarily encumbering our nomenclature. The truth is, that serous inflammation in the pelvis is rarely, if ever, confined to the peritoneal covering of one organ; and, besides, we have no means of diagnosing it when it is. In those facts we find other contra-indications to the employment of a term so restricted in its meaning as perimetritis.

3*d*. The preposition *para* means, by the side of, and to most Greek scholars a word like parametritis would indicate too close a proximity to the uterus to be used with correctness in a case, for example, of puerperal cellulitis ‡

* Paranephritis is used by Dunglinson, for instance, to express Inflammation of the Suprarenal Capsules.

† Aran, Leçons Cliniques sur les Maladies de l'Utérus.

‡ I am perfectly well aware that Dr Duncan denies the extension of a mere cellulitis etiologically parametric, as he would say, to the iliac fossa. This is a point which can only be settled by further inquiry, and especially by autopsic demonstration. I can only say that I imagine I have seen a case of iliac cellulitis ending in abscess from an injury to the uterus without any matter having formed in the true pelvis; and a case adduced by Dr Duncan himself, at p. 155 of his work on Perimetritis and Parametritis, seems to refute his theory. (See also Bernutz and Goupil, Clinical Memoirs on the Diseases

in one or other of the iliac fossæ, a difficulty which the term pelvic cellulitis to a great extent removes. But besides, the preposition *para*, according to our best Greek authorities, such as Donaldson and Jelf, is never used to indicate proximity to a person or thing, except with the idea of motion towards or by the side of that person or thing.

4th. The terms pelvic cellulitis and pelvic peritonitis show to any one, at a glance, the nature of the affection, whether it is inflammation in the cellular tissue or serous membrane of the pelvis, and do not limit the seat of the disease to the immediate neighbourhood of the uterus, as I maintain the words perimetritis and parametritis must do. If we keep in remembrance that the terms pelvic cellulitis and peritonitis are almost invariably employed by gynækologists when the inflammation in the pelvis arises from some kind of disease of, or injury to the uterus and its appendages, I hold that, at least in the present phase of our knowledge of the affections, those names can produce no confusion whatever in the mind of any one who adopts them, but must necessarily assist him in rapidly forming a conclusion as to the seat and pathology of the disease.

Up to a very few years ago the pathology of pelvic inflammation was only partially elucidated, or had been in many cases erroneously interpreted. Some facts, apparently well established, and, at any rate, supported by the weight of great names,* pointed to the cellular tissue as the seat of those inflammatory processes which were so frequently known to follow the act of parturition, and even to occur in the non-puerperal female from many and varying causes. But as our knowledge of pathology advanced, it came to be recognised that many of those cases which had been regarded as purely extra-peritoneal, were in reality affections of the serous membrane itself and the consequences arising therefrom, and that, in fact,

of Women, translated for the New Sydenham Society, by Dr Meadows, vol. ii., p. 146.)

* Sir J. Y. Simpson, Dr Churchill, Dr West, Nonat, etc.

we had to deal with two perfectly separate and distinct diseases, which, indeed, might complicate each other, but of which, strange to say, the one first carefully described* was by far the most rare, and by much the least important.

It would be unjust to one of the most careful and conscientious of clinical observers, M. Bernutz, not to mention that it was principally to his researches that we are indebted for our increased knowledge of the true nature of those inflammatory processes, although I am by no means prepared to go so far as he does, and almost absolutely deny the occurrence of cases of perimetric cellulitis at all. The fact seems to be, that while the theory of inflammation of the cellular tissue in the pelvis—a theory almost universally accepted in our own country—was carried too far by the enthusiasm of Nonat in France, M. Bernutz committed an equally grave error by rushing much too blindly in the opposite direction. The truth, as it usually does, appears to lie within the extremes; and though we can easily believe that M. Bernutz has verified his diagnosis more frequently by autopsies in cases of pelvic peritonitis, it must not be imagined, on account of the rarity of such post-mortem verification, that cases of perimetric cellulitis are uncommon.

On the contrary, it may be pointed out, that the very fact of the lesion being so frequently extra-peritoneal is the reason why we so seldom have an opportunity of completing the diagnosis in the way mentioned. No one can for a moment doubt that pelvic peritonitis is a much more serious and grave lesion, and one much more likely to produce a fatal issue than inflammation of the cellular tissue of the pelvis, and of that alone; and while, on this account, we have seldom any opportunity of confirming the presence of a perimetric cellulitis by post-mortem inspection, such a case as that reported by M. Simon†

* Grisolle mentions cases of circumscribed pelvic peritonitis simulating phlegmons as early as 1839, but there was really no accurate information until the papers of Bernutz appeared.

† Bulletins de la Société Anatomique de Paris, xxiv. année, p. 234.

goes far to prove, not only the possibility of its existence, but even the probability of its frequent occurrence.

From this short *resumé* of the present state of our knowledge of those essentially distinct pelvic inflammations, I now turn to one or two cases which will partially serve to illustrate the symptoms to which they give rise, the treatment we ought to adopt, and the post-mortem appearances in such as terminate fatally.

Margaret H., æt. 39, married, and has had four children, was admitted into Ward XII.,* Royal Infirmary, on 6th March 1869, on account of hæmorrhage after an abortion, which had occurred four weeks previously. For purposes of diagnosis the os and cervix were dilated by sponge-tents, and, after examination, the ergot of rye was given in drachm doses of the liquid extract, thrice daily. The bleeding ceased, and she was about to leave the hospital on 25th March, when she was seized with a rigor after lying in her bed with the adjacent window open. The pulse and temperature rose very considerably. She perspired profusely after the shivering, and became delirious. She also complained of pain above the pubes. The next day, on vaginal examination, there was fulness and tenderness in the retro-uterine cul-de-sac, but nothing more. Ordered opium internally, and poulticing. On the 29th the pain was not quite so severe, and was relieved by her drawing up her knees, and thus removing the tension of the abdominal walls from a slightly tympanitic abdomen. Immediately above the pubes a somewhat hard and very tender swelling was felt, but its limits could not be made out on account of the pain. Vaginal examination proved the existence of a swelling in the posterior cul-de-sac pressing the uterus forward against the pubes, and, by combined internal and external palpation, it was found to be part of the same swelling felt above the pubes. The pulse was 120 per minute. She was perspiring very much, but complained less of the pain.

* Under the charge of Sir J. Y. Simpson.

On the 5th April the swelling seemed softer, and apparently indented slightly on pressure. An exploring needle was introduced, and as pus escaped through it, a tenotomy knife was at once employed to make an incision in the most prominent part of it. The cut was immediately behind the cervix, was nearly an inch in length, and through it there escaped very freely about a pint and a-half of very fœtid pus. On introducing my finger through the opening, I felt what I considered to be the peritoneal recto-vaginal fold below. The swelling above the pubes still to be felt, but neither so large nor so sensitive.

I need not enter into all the minutiæ of her case given in the Ward Case-book; suffice it to say, that by the 9th April she had decidedly improved. There was little pain, and she could keep the legs stretched out in bed. The opening internally was closing, but the uterus was fixed and slightly anteflexed, and there was fulness and tenderness in the right lateral and posterior regions. By the 16th of the month there was no suprapubic pain remaining, but the uterus, though more mobile, could not be pressed without causing a shooting pain through the abdomen. The improvement continued until the 25th, when she had an attack of an epileptiform character, and it turned out that she was liable to similar fits. After them she feels very drowsy, has a vacant, expressionless stare, and does not answer correctly questions put to her. However, after this attack she steadily gained ground, and was dismissed on 1st May, declaring herself to be quite well, though the uterus was still pretty immobile and anteflexed, but somewhat to the left. The wound was quite healed up, and there was no discharge.

This was a highly characteristic case of adhesive pelvic peritonitis following abortion, and ending in abscess. The rapid onset of acute pain, the high degree of inflammatory fever, and the great prostration of the patient from the beginning of the attack, seemed to point to more serious mischief than simple inflammation of the pelvic cellular tissue. Still, many of the symptoms we are accustomed

to find in other cases of pelvic peritonitis were wanting in this patient. As in all peritoneal affections, we often have from the commencement of the attack distressing nausea and vomiting, in consequence of which the pulse soon becomes small and quick. Dysuria, occasionally amounting to strangury, and sometimes even leading to complete retention, is often most violent, and I have seen one or two cases in which the attacks of spasmodic pain, apparently at the internal orifice of the urethra, have been so acute as to deprive the unfortunate sufferers of all power of self-control, and to lead them to utter shriek after shriek in their agony. Tenesmus, too, is another of the most distressing of the painful symptoms present, and great pain in defæcation is almost unavoidable, from the ordinary situation of the swelling and effusion. It need scarcely be added, that all appetite is usually lost; that frequently great thirst annoys the patient, and that she lies prostrate in bed, afraid even to stir, and unable to bear the lightest pressure on the wall of the abdomen, and that sleep is but seldom obtained without the use of anodynes. If the inflammation progresses, as it sometimes does, and extends to the abdominal serous membrane, the symptoms of general peritonitis rapidly supervene, and the patients usually succumb. If, on the other hand, the inflammation remains limited, the symptoms become those of chronic pelvic peritonitis, or change into those of intra-peritoneal pelvic abscess.

Still, I do not wish to be misunderstood, nor to lead any one to imagine, because, in a typical case like the one reported, little chance of mistake can arise, that therefore we are able in every instance to make an exact diagnosis. It is only too true, unfortunately, that, in the present state of our gynækological knowledge, we cannot point out any single pathognomonic feature to distinguish pelvic peritonitis from the closely allied pelvic cellulitis; for even though the greater gravity of the symptoms in most cases of pelvic peritonitis cannot be denied, yet it would be highly improper to rely on such a fact for our

differential diagnosis. I have seen as severe pain and other symptoms of high inflammatory action in cases of pelvic cellulitis, as I have in cases in which I had every reason to suppose the serous membrane alone affected; while, on the other hand, I have been frequently convinced of the existence of sero-adhesive pelvic peritonitis, without the patient presenting such symptoms as would lead us to suspect inflammation in any part of the peritoneum. No doubt, the two affections may often be present coincidently; and, in fact, I have little hesitation in saying, that I believe cases of pelvic cellulitis are very frequently accompanied by some slight peritonitis. Here is a case in point:—

Mrs C., æt. 40, from Liverpool, residing in Lauriston Place, came under my care towards the end of March 1869. Her previous medical history was not very good. She had been married fourteen years, and a year after marriage had a child, but did not make a good recovery, and has never felt very well since. She had been treated by various medical men; had taken a good deal of medicine, especially aperient, and had occasionally improved. She came down to Edinburgh, and was found to have retroflexion of the uterus, for which a loop pessary was introduced. She went about wearing this, although it gave her great pain; and it was only at the end of eight days after its introduction that I was again called to see her. Her symptoms were not by any means marked, the pulse not being above 100, but she could not go about on account of pain referred to the sacrum. On vaginal examination, a swelling between the vagina and rectum was discovered, painful to pressure, but not yielding any sense of fluctuation to combined rectal and vaginal palpation. The uterus was high in the pelvis, not immobile, however, at this time. The cervix was pressed close to the pubes. A few days after my first visit, I found her complaining of supra-pubic pain; and, on examining this region, discovered a tender swelling, which projected slightly above the pubes. Although the swelling behind

the vagina was more prominent and nearer the anus, I managed carefully to pass a sound, and proved that this tumour was the fundus uteri. On the eighth day after the first examination, combined rectal and vaginal palpation showed the existence of fluid in the swelling, and I opened it freely with an ordinary bistoury through the posterior wall of the vagina. A good deal of pus escaped, and the tumour above the pubes at once disappeared. On passing my finger into the abscess, I found that it reached, in the cavity formed in the cellular tissue, nearly to the anus. The pus continued to flow for about a week, and a month afterwards she left Edinburgh, having made an excellent recovery, and declaring herself to be freer from pain and uneasiness than she had been for years. Her state then, as given in my note-book, was as follows:—"No pain from deep pressure in the supra-pubic or iliac regions. Internally, uterus pretty immobile and acutely retroflexed, but somewhat laterally towards the left sacro-iliac synchondrosis. Pain on pressing the cervix upwards, and there is still some slight ulceration of the os uteri. She eats and sleeps well, and, though weak, is able to walk about without pain. The incision is quite healed, but there is some fulness remaining in the recto-vaginal space."

The situation of the cellulitis in this patient between the rectum and vagina is by no means the most common. West, in his work on the *Diseases of Women*, placing only fourteen out of fifty-one cases in this position. The most common seat of cellulitis is said to be within the folds of the broad ligaments; but, even setting aside the difficulty of accurately ascertaining the exact site of those inflammatory products without a post-mortem examination, it must be remembered that Nonat, West, and M'Clintock, who give statistics or offer opinions on this subject, do not sufficiently discriminate between the intra and extra peritoneal forms of pelvic inflammation, thus rendering any assertions on the point almost valueless. For similar reasons, it must be equally apparent that we have at the

present no reliable information on the subject of the comparative frequency of suppuration in the two distinct forms of inflammation in the serous membrane or cellular tissue of the pelvis. Thomas, indeed, with a boldness worthy of a better cause, roundly asserts that the marked tendency to suppuration in cellulitis, is an important point in the differential diagnosis of that affection from peritonitis; but, though other authors, as Duncan, maintain that suppuration is the most common termination in cellulitis, yet no one, so far as I know, except Thomas, has ventured to draw any important inference from what is at the best a mere supposition on his part.

There can be no doubt, however, that when pus has formed in the cellular tissue, it much more frequently finds an exit for itself externally than internally. The statistics of M'Clintock—though liable to correction on account of the fact just stated—seem to show that it is through one or other of the iliac regions that the puerperal cellular abscess most commonly discharges itself. But when M'Clintock goes on to express his concurrence with Dr Bennett in the statement, that non-puerperal pelvic cellular abscesses are most frequently discharged through the rectum or vagina, I cannot help affirming that he offers no adequate proof for his belief. The passage, however, in M'Clintock's essay on the subject is so confused, that I am quite unable to make out whether he is really speaking of cellular abscesses only, or whether he does not include intra-peritoneal collections as well. "I have an opinion," says Duncan,* "that abscesses in the cellular tissue (parametric abscesses) open more frequently externally than intra-peritoneal abscesses (perimetric abscesses) do;" and Aran seems to be much more convinced of the truth of the assertion than Duncan is. At least he states very decidedly his opinion, that intra-peritoneal abscesses almost never find an exit through the abdominal walls; and on anatomical grounds alone, we may be fully as confident of this as Aran. The fact seems to be, as Duncan

* On Perimetritis and Parametritis, p. 160.

has already pointed out, that the intra-peritoneal abscesses perforate the most perforable viscus; and if we reflect for a moment on the most common site of the purulent collection in those cases, it becomes evident that the rectum and sigmoid flexure are the parts of the intestines into which the matter is most likely to find its way. Very seldom, indeed, does the perforation occur through the bladder, and even more rarely through the uterus or vagina.

That suppuration is more common in the puerperal* than in any other variety of pelvic inflammation—whether in the serous membrane or cellular tissue—is a fact disputed by no one; and I think it also probable, that not only is suppuration much the most common termination in such cases, but also that the matter forms much more rapidly. In one patient, in whom the peritonitis had been caused by exposure to a cold wind after an abortion, the pus was completely matured, and the abscess ready for opening on the seventh day after the existence of fulness in the retro-uterine cul-de-sac had been made out. There are one or two other events, too, which have been noticed by observers as more frequent in the puerperal form of the disease than in any other. M'Clintock, for example, calls our attention to the very insidious manner in which the attacks sometimes come on; and in reference to this makes the admirable observation, that in all cases of bad recovery from childbed, we should carefully examine the iliac regions day by day, as he has thereby frequently been enabled to detect latent inflammation when no other symptom indicated the occurrence of morbid action. In making this remark, he is, of course, speaking of cellulitis, the only variety of pelvic inflammatory tumour he attaches any importance to; but if we add, that in many such suspicious cases, it is absolutely necessary to have recourse to internal examination, the observation is equally valuable with reference to pelvic peritonitis. Bernutz, too, notices

* Under this head, of course, are to be included the cases coming on after abortion.

another danger to which the puerperal patient is much more liable than the non-puerperal, when pelvic peritonitis has been developed. "The extension of the inflammation by simple contiguity from the pelvic to the abdominal peritoneum," he observes, "is especially liable to occur in cases of puerperal pelvi-peritonitis at about the end of the second week when the patient has not taken sufficient care of herself. This happens much more frequently in hospital than in private practice." The following notes of the post-mortem examination* of a patient who died in the Royal Maternity Hospital, tend to show that this observation of Bernutz is unfortunately only too often verified:—

The patient, aged 29, miscarried at the seventh month, and was delivered of a dead child, after a labour lasting nearly forty hours. She died ten days afterwards, her symptoms all along having been very obscure, and the termination sudden. At the autopsy the cause of death was found to be general peritonitis, but the important point as bearing on our subject, was the fact that the posterior part of the pelvis was hollowed out into a large cavity, containing about a couple of pints of sero-purulent fluid, which had principally gravitated into the lower part of the retro-uterine cul-de-sac. This cavity was formed by the still enlarged uterus, ovaries, and broad ligaments in front and to the sides, by adhesions of the fundus uteri to the intestines above, and by the rectum and peritoneum behind. The adhesions were recent and easily removed, and the rectum and posterior surface of the uterus were covered by recent lymph. The internal surface of the uterus presented the ordinary appearance of a puerperal uterus. There was no endometritis and no inflammation in the mucous membrane of the right or left Fallopian tubes. The posterior surfaces of both broad ligaments were covered by lymph. The right ovary was much enlarged,

* The case has already been brought under the notice of the Obstetrical Society by Dr C. Bell, and will be found at greater length in the *Edinburgh Medical Journal* for August last.

being two inches in length. On its anterior surface, and close to the Fallopian tube, a small rounded orifice about the size of a fourpenny-piece was found. Its edges were smooth. On the lower border, another, but larger, rounded orifice existed. It was about the size of a shilling, and had ragged and everted edges. These two orifices communicated with each other, and the ovary between was excavated out into an irregular cavity, which, however, contained no pus.

It cannot be denied that cases similar to this have frequently occurred, and it is almost equally certain, that in very many of those cases, and up to a very recent time, so much confusion existed in the minds of observers as to the nature of the malady, that the patients attacked with pelvic peritonitis after delivery were supposed to be labouring either under metritis, or, in bad forms, under one or other of the manifold forms of that Protean disease —puerperal fever. No doubt, in nearly every case of what is called puerperal fever, we have pelvic peritonitis, but this is only one of the many local manifestations of a constitutional state. On the other hand, in such a case as the one just reported, we can usually refer the inflammation to some specific cause; and the existence of septicæmia, or the presence of a contagious element, do not enter into our speculations as to its origin. Thus, we see at once that it was the bursting of a small intra-ovarian abscess, during a lingering labour, that gave rise to the peritonitis, which, by extending to the abdominal serous membrane, caused the death of this patient.

There can be no doubt that all the forms of intra-peritoneal abscess, from whatever cause arising, are incomparably more dangerous than those resulting from pelvic cellulitis; and even though the matter be favourably situated for evacuation, we ought in such cases to have a constant dread of general peritonitis, and its almost invariably fatal issue, and we must use every means in our power to avert such an unfortunate result. Those abscesses, it is true, are usually encysted within walls,

formed either by fibrinous deposit, or by the matting together of two or more of the pelvic or abdominal viscera; but even though such walls may be very useful in preventing the lighting up of a general inflammation, yet the constant production of matter from the newly-formed pyogenic membranes is very apt to lead to the exhaustion of the patient, especially if the pus has found an outlet, whether externally or internally. We must always bear in mind, too, that the walls of abscesses formed in this way are, at any rate at first, not very strong, and that instances have occurred where rough handling, either through the abdominal parieties or per vaginam, has ruptured the fragile membranes, and caused the exact result we ought so carefully to avoid. Another danger which often puts the lives of such patients in jeopardy, in whom the abscess opens into the bowels, is the long-continued exhaustion which sometime results from the chronic diarrhœa or dysentery thus produced; nay, more, I have a most vivid recollection of the unutterable agony undergone by one lady from the frightful attacks of tenesmus, caused by the passage of such acrid matter over the sphincter of the anus,—an agony which, yielding to no remedy but chloroform, was in itself sufficient to have killed the unfortunate sufferer.

In every attempt which has as yet been made to classify cases of pelvic peritonitis according to their causation, the puerperal variety, including those cases coming on after abortion, has been universally allowed to be most frequent. The truth is, however, that though more than half the cases would be found to come under this head, yet we can seldom be quite sure, without the help of an autopsy, of the primary disease which has given rise to the pelvic peritonitis, and as to whether that primary disease is uterine, tubal, or ovarian. Some authors, certainly, do not seem to have much doubt on this subject. Thus, Aran, in his *Leçons Cliniques sur les Maladies de l'Utérus*, most distinctly gives it as his opinion that the primary

affection is almost invariably ovarian or tubal. At p. 663* of that work, he says, "The true element, that playing the principal part in what we call periuterine inflammation, is the alteration of the uterine appendages, the ovary, or tube." Again, at p. 673, "Perhaps the inflammation of the uterus, especially endometritis, plays some part in the invasion of periuterine organs by inflammation, independently of any alteration in the ovary or tube. This I can say, that I have as yet met with no fact of the kind,† and am not completely convinced by any known facts which might seem to support an opposite opinion. It has not been shown to my satisfaction that the ovaries and tubes have been examined with sufficient care. Too frequently the examination has been made months, nay, years, after the attack, when any traces of inflammation in these organs may have disappeared long before." Again, at p. 679, we find him making no less positive statements as to the manner in which the inflammatory swellings are formed. "When the appendages of the uterus are affected only on one side," he says, "it is from those appendages, as from a centre, that the inflammatory changes extend, and the situation of the diseased ovary or tube fixes definitely the site of the tumour in front, behind, or on either side of the uterus. But if, as in a number of cases, the appendages are attacked on both sides at once, the pathological changes soon meet in the mesial line, most frequently behind, but sometimes in front of the uterus, surrounding that organ in a kind of ring, which is more or less complete." From these quotations, and from many other passages of the work, in which the same idea is reiterated, it will be seen that M. Aran makes the uterus play a very subordinate part in the development of pelvic peritonitis; and Dr West, in his admirable treatise on Female Diseases, takes up the same notion, and expresses the same opinion in

* See also pp. 667 and 718.

† Dr M. Duncan has, however, managed to convict M. Aran of inconsistency in his statements. Vide *Duncan on Perimetritis and Parametritis*, p. 41.

almost identical terms. But we can scarcely hesitate to believe, that in many of the cases the uterus is the real starting-point of the inflammation. Indeed, it has lately been stated* that the extension of endometritis along the tubes is the most common cause both of the puerperal and non-puerperal varieties of the malady we are discussing. However true this may be of the puerperal and gonorrhœal forms of pelvic peritonitis, I am not as yet inclined to believe in the frequent causation of other varieties in this way. I have seen limited inflammation in the serous membrane so rapidly developed after the introduction of a tangle-tent, the employment of a cutting or burning instrument to the uterus, or even after the use of the sound, that no inflammation in the mucous-lining† of the interior of the womb could have had time to spread through the tubes to the peritoneum.

That the determining causes of the puerperal variety of pelvic peritonitis are much the same as those that might give rise to metritis or endometritis, may be adduced as an argument in favour of the opinion that the affection of the serous membrane is merely an extension by contiguity of the inflammation in the womb. Out of eighteen cases given by M. Bernutz, we find him stating that four arose from difficult parturition; three from cold; nine from too early rising after confinement; one from fatigue; and one from venereal excess; and M'Clintock observes the same fact as to the frequency of its origin from too early rising, as he mentions that Dr Heyerdahl of Christiania had informed him that, so frequently were patients attacked wtih pelvic cellulitis after leaving the Maternity Hospital in that city, that the managers had felt themselves compelled to lengthen the duration of

* Duncan, *op. cit.*

† I do not here enter into the question whether metritis itself can be propagated by contiguity to the peritoneum. Metritis of the non-pregnant or non-puerperal uterus must, indeed, be rare when Klob declares that he has seen no case in which he could positively say a "genuine metritis" existed. (Klob, Pathologische Anatomie der Weiblichen Sexualorgane, s. 209.)

residence in hospital, to prevent, if possible, such accidents in the future.

Bernutz dwells more particularly on the fact of the extension of the inflammation from the mucous membrane, when he enters on the consideration of what he calls blennorrhagic pelvic peritonitis, and he concludes, from his observations in the Lourcine Hospital,* that it is a very common variety, indeed, and that pelvic peritonitis from this cause is not only the analogue of orchitis in the male, but also that it occurs in about the same proportion of cases as does that extension of gonorrhœal inflammation in men. Nay, so far does Bernutz drive his analogy, that he even occasionally gives to the whole group of symptoms of a case of pelvic peritonitis the name of *feminine orchitis*.†

Very few physicians in this country have had the same opportunities of studying the gonorrhœal variety as Bernutz has had at the Lourcine Hospital; and, in consequence, we find very few cases of blennorrhagic pelvic peritonitis in the writings of our gynækologists. So far as I can remember, I have only seen a single case of this interesting variety, but that a most formidable one, though it did not end fatally. In this woman I had the opportunity of treating the case from the commencement of the vaginitis to its termination in oophoritis, and inflammation of the serous membrane round the ovaries, and of tracing the gradual progress of the disease through its successive stages. It did not differ materially from any of the cases of a similar nature given in the work of Bernutz, except that the pelvic peritonitis was not developed, or, at least, was not evident until fully three months from the time when the gonorrhœa had been contracted,

* The Lourcine—the Female Syphilitic Hospital in Paris—offers, of course, exceptional statistics on this subject. During several months of the year, Bernutz tells us that, owing to want of space, they were obliged to admit as in-patients only those who presented the symptoms of periuterine phlegmons.

† Properly speaking, it is the oophoritis which is the analogue of orchitis, but it is impossible in such a case to separate the inflammation of the tissue of the organ from that of its serous membrane.

whereas the observation of Bernutz has not as yet presented him with any case in which the lighting up of the serous inflammation was delayed more than two months from the first appearance of the purulent discharge. In relation to this point, however, Dr Duncan judiciously observes that the dangerous period after delivery, after the commencement of a gonorrhœa, or after an uterine operation, lasts much longer than is generally supposed,—an observation, the full importance of which can only be appreciated by those who remember its practical bearings on prognosis and treatment.

Amongst the different forms of pelvic peritonitis, one of the common, and one of the most important, is the menstrual, under which head Bernutz includes 20 out of his 99 cases. In the majority of those patients the inflammation arises from the sudden suppression of the menstural flux, whether that be caused by cold, by some mental emotion, or by any injury during the period. In almost all such cases which terminate favourably, it will be nearly impossible for us to detect the part primarily affected, although, as we believe, there must necessarily be some starting-point or other for the inflammatory action. In the following case I suspected that the right ovary was the *origo mali* :—

Mrs G., æt. 30, from Forfarshire, was suddenly attacked on the second day after the commencement of the menstrual period with most violent pain in the right side of the pelvis, and when I saw her a few hours afterwards was labouring under symptoms referable to pelvic peritoneal inflammation.* There was nothing to be made out of external examination, and on account of the menstrual flow I made at that time no internal explora-

* I had seen this lady about a week before, when she had complained of some pain over the cæcum, but this had yielded to very mild remedies, and she had been going about and feeling perfectly well until the sudden attack during the menstrual period. I was called to attend her in the absence of Sir James Simpson on the Continent.

tion; but the next day I discovered by palpation a small and very tender swelling above the right supra-pubic region. Internal examination—the period ceasing the same day—discovered the uterus nearly immobile in the pelvis, its fundus anteflexed, and at the same time drawn over to the right, and the cervix high up and looking towards the left. There was great tenderness in the right lateral region and posterior cul-de-sac, and a feeling of fulness, but the left side was quite free. Any pressure on the cervix at once caused a darting pain in the seat of the swelling to be felt through the abdominal walls. The bladder and rectum unaffected. Pain very excruciating, and only relieved by repeated subcutaneous injections of morphia. The swelling increased rapidly in size for the next few days, until it reached about three fingers' breadth above the pubes, at the same time extending outwards towards the anterior superior spine of the illium. The pain continued most violent, and all the ordinary symptoms of peritonitis were present. The pulse was small and rapid, keeping about 120 per minute, and vomiting, which began on the third day, and lasted for about twenty-four hours, seemed to yield finally only to small doses of dilute hydrocyanic acid. About the fifth day, and coincidently with the subsidence of the vomiting, the pain became more endurable, the pulse sank to about 100, and the tension of the abdominal walls and tympanitis diminished. The tumour, though still very tender to the touch, could then be felt from without to be distinctly circumscribed, and did not extend more then two inches beyond the symphysis. Percussion over it elicited a slightly tympanitic sound, except just above the pubes, where it was also most tender to pressure. After the sixth or seventh day from the commencement of the attack the lady did very well, but the pain remained very obstinate, and was always worst down the front and inner side of the thigh. Still it was not relieved in any degree by keeping the thigh flexed. The swelling began slowly to disappear, and on internal examination,

about the twelfth day of her illness, the uterus was found to have regained some of its mobility, though pressure on its cervix or up against the fundus still caused the violent shooting pains already described. The ante-lateral flexion remained the same.

On the sixteenth day from the beginning of the attack, I found that the swelling was scarcely to be felt through the abdominal wall, except by deep pressure into the pelvis, which still caused the patient very great pain. The uterus in much the same state, but its mobility undoubtedly greater. She was at that time eating and sleeping fairly, but had begun to complain of pain in micturition. She continued slowly to improve, and there were no symptoms of suppuration present, when, rather to my surprise, on the twenty-fourth day pus appeared in the water in very large quantity; and a day or two later nausea and vomiting began again, and proved most obstinate, resisting all the various methods of treatment I will presently detail. She had ultimately to cease taking any food or liquid by the mouth, being nourished solely by nutritive enemata. Her pulse, however, during this time never rose above 100, and she managed to get a fair amount of sleep, though chiefly by means of the subcutaneous injections. The pus continued to appear in the urine for about a fortnight, gradually diminishing, however, in quantity—the acts of vomiting, too, becoming much less frequent; and, without entering into further details, she was convalescent, and able to be out of bed about the middle of the seventh week. The uterus had then regained much of its mobility, but was still anteflexed. A month or two afterwards she went to travel on the Continent, and when I saw her on her return, she told me she had not suffered in any way from fatigue or exertion. I ought to have mentioned, that this lady had only had one child some ten years before, when she was delivered with great difficulty. The labour occurred in Italy, but she could give me no particulars of it, except that it lasted three days. She also stated that she had

suffered from similar attacks to the one I have just described, but not so severe.

The swelling—for, strictly speaking, it could not be called a tumour—which formed so rapidly in this patient was obviously formed by certain portions of bowel agglutinated together by lymph, and at the lower part probably adherent to the right ovary, and possibly also to the fundus uteri, which was drawn over to that side. That the swelling was mainly formed by intestine is evident from the fact that percussion over the upper part of it elicited a modified tympanitic sound, which changed into a dull note nearer the pubes. Indeed, the hard and resistent tumour we find in similar cases of pelvic peritonitis is mainly due to the matting together of the pelvic viscera by the deposited lymph, which, becoming stronger and firmer, produces very dense fibrous adhesions; and also to the accumulation of sero-purulent or purulent fluid in the loculaments formed by such bands. It is owing to these facts that chronic pelvic peritonitis has not unfrequently been confounded with ovarian and fibroid tumours.

The reasons which led me to suppose that the right ovary was the original cause of the inflammation in the serous membrane in this patient are chiefly these:—

1*st*. The occurrence of the attack at a time in which the ovaries are especially the seat of congestion.

2*d*. The position of the swelling.

3*d*. On internal examination, after the subsidence of all inflammatory action, I discovered an enlargement to the side of the uterus, which was in the position of the right ovary, and still sensitive to pressure.

4*th*. The statement of the patient that she had had four similar attacks at periodical intervals of two years, and that on each of these occasions she had suffered from the same violent pain chiefly in the right side and leg.

It may, however, at once be stated, that we never can be positively certain of the real cause of the attack without the aid of an autopsy, and even not always then, as

the traces of the original disease may either have completely disappeared, or have become merged in those of the secondary affection.

Amongst the many forms of pelvic peritonitis, the traumatic variety does not take a very prominent place in the systematic works on the subject, although, in my opinion, its importance is considerably underrated. I am compelled to express my conviction also, that the inflammation of the serous membrane in many instances of this variety is due either to some imprudence on the part of the patient, or to some want of caution on that of the physician, although cases of course occur in which neither would seem deserving of blame. Thus, I distinctly remember, that in one patient the pelvic peritonitis was apparently produced by the passing of an uterine sound to measure the cavity. The peritonitis in this case—a weak exhausted woman of about forty—spread rapidly to the abdominal serous membrane, and proved fatal in some eight or ten days. At the autopsy no sufficient cause for the development of the peritonitis could be found; there was no endometritis or inflammation in the Fallopian tubes.

Among other determining causes of the traumatic variety, I may mention the use of tangle-tents to dilate the cervix, several examples of pelvic peritonitis arising in this way having come under my notice. Cutting operations on the uterus, the excision of polypi, the application of solid nitrate of silver to the interior of the womb, or of caustic potash to the cervix, and the wearing an intra-uterine pessary, have all proved the exciting causes of such cases; and among others which have been mentioned, and which we can readily conceive may have produced the inflammation, we find recorded venereal excesses, injections of fluid into the uterine cavity, and chancres on the cervix.

I cannot conclude these observations without making some allusion to the important subjects of diagnosis and treatment, and it is with some shame that I confess that much, very much, remains to be done before we will be

able to publish any incontrovertible opinion on either point. The greater number of gynækolgists, in attempting to establish a differential diagnosis between those affections, of the serous membrane, and of the cellular tissue in the pelvis, either simply repeat the leading signs and symptoms they imagine to be peculiar to each, or even fail more lamentably by ambitious, if praiseworthy, attempts to establish certain points in the diagnosis, which they consider to be infallible. In a work * only very recently published, we may see an instructive example of the complete failure of such an experiment. Thomas has drawn up a long series of differential points, of which it may be said that in some cases his statements would answer for both complaints, while in many others they might be reversed without impropriety. As no one, then, can point out any one or two features so salient as to guide us by their existence to the immediate separation of the one form of inflammation from the other, it would perhaps be as well to abstain from doing so at all, lest in attempting to avoid the Scylla of overpositiveness we fall into the Charybdis of obscurity. I shall endeavour, however, in as brief a space as possible, to indicate the direction at least in which our attention must be turned when we are trying to separate the two affections.

In a former part of this paper, I expressed a belief that we would be committing a grave error if we were only to rely on the greater severity of the symptoms in cases of pelvic peritonitis than of cellulitis; and, in repeating this statement, I am necessarily forced further to point out that it is to the physical signs we must chiefly trust to guide us correctly; and certainly I can confidently say, that there are few more difficult points in physical diagnosis than this, the difficulty being in many cases greatly increased by the fact that, unless under chloroform, the combined internal and external manipulation necessary to satisfy us is usually at first too painful for the patient.

* Gallard Thomas, A Practical Treatise on the Diseases of Women, p. 375. (Phil. 1868.)

In by far the greater majority of cases, then, of pelvic cellulitis, we find, on internal examination, that the induration or swelling is on one or other side of the uterus, and that it is only very rarely that the whole roof of the pelvis seems blocked up by the hard stony mass so common in cases of pelvic peritonitis. The tumour in most cases of cellulitis is found principally in one or other of the iliac fossæ, and apparently closely connected to the iliac bones, or, if it has not advanced so far, to the pubes; while the tumour, if felt at all through the abdominal walls in peritonitis, is usually more in the mesial line, or, if situated laterally, does not by any means seem so intimately blended with the bones. An important point of the diagnosis of pelvic cellulitis, as well as of pelvis peritonitis, is the immobility or fixation of the uterus—an immobility which, in many cases of peritonitis when it is fixed all round, may be said to be absolute, while in cellulitis it rarely if ever happens that the fixation is so complete. As the immobility arises to a great extent from the presence of adhesions uniting the womb to other organs or viscera, and not alone from the presence of what we may call a tumour in the pelvis, we can easily conceive why it should be more marked in inflammation of the serous than of the cellular tissue.

In cellulitis, the uterus is usually pushed to the opposite side of the pelvis from that occupied by the phlegmon, whereas, in some cases of peritonitis, the body and fundus of the womb are so completely lost or enveloped amidst the exudation* that it is only by the use of the sound, when such a use is justifiable, that we can determine its exact position in the pelvis. There can be no doubt, too, that flexions of the uterus of one kind or another—ante, retro, or lateral flexions, according to the position of the swelling—which are common in peritonitis, occur much more rarely in cellulitis. When the swelling has risen into the abdomen, we may be partially assisted in our diagnosis, not only by observing the position of the

* See Aran, *op. cit.*, p, 680.

tumour, or by having followed its mode of origin and progress, but also by calling to our assistance such aids as percussion and auscultation. Portions of the intestine so frequently enter into the composition of the intra-peritoneal tumours, that we can often obtain from them a more or less modified tympanitic note, or hear the movements of gas or fluids through the mass. I know of only one form of cellulitis that could be confounded with an intra-peritoneal tumour under those circumstances, and that is, the extension of the phlegmon to the abdominal cellular tissue, the *external peritonitis* of some authors. There is one point which every gynækologist can corroborate—namely, the retraction of the thigh in cases in which the cellular tissue round the psoas and iliacus muscles is involved, and the relief of pain which this flexion produces. I have frequently noticed, in cases of pelvic peritonitis, that the flexed position of the thigh either did not ameliorate the pain, or did so only when both knees were raised, by removing the tension of the abdominal muscles.

To go any further to attempt the separation, for instance, of the different varieties of pelvic serous, and cellular inflammation from one another, or those again from other diseases for which they might be mistaken, would lead me far beyond the scope of my present observations. I have said enough, I hope, to show that, while in a few cases the most experienced practitioner may without shame confess himself at fault, yet, in the majority, the whole of the features of the case, the *tout ensemble* of signs and symptoms, present to our minds so many points of sufficient distinctness as to enable us to form a correct diagnosis.

If most of our gynækological authors hesitate, however, to lay down any definite rules to guide us in our diagnosis, many of them are, on the other hand, rather too dogmatic on the subject of treatment. In this country, at least, it is not likely that many of us will be inclined to follow the rules which Nonat considers so infallible,

and adopt his *coup-sur-coup* plan of bleeding, or to assent now-a-days to Aran's doctrine of the benefit to be derived from mercurialisation. There may still be among us practitioners who think that we have rushed too blindly from one extreme into the other, and who remind us of the great alleviation of pain that bloodletting brought with it; but certainly there must be few, very few, left who can unhesitatingly approve of the decidedly antiphlogistic treatment of one section at least of the French school of medicine. As I must emphatically reject, then, any such violent plans of treatment as have been just mentioned, it appears necessary to say a few words as to the method which it seems most advisible to pursue—a method which is based chiefly on the doctrines taught in our own University.

Before doing so, I wish to remark, that many of the cases of pelvic inflammation might possibly be prevented by a proper regard to prophylactic measures, and it is in this respect that the observation already quoted from Dr Duncan's work on Perimetritis and Parametritis seems so valuable.* The experience of the Directors of the Christiania Lying-in Hospital, as reported by M'Clintock, proves to us, for instance, how necessary it is both to attend to puerperal patients, by preventing them returning to work too soon, and not to neglect even the slightest anomalous symptom occurring in women in this state. Many cases, again, of the gonorrhœal variety might have been averted, if the practitioner had properly interpreted the indications which the extension of the specific inflammation to the mucous membrane of the womb presented. I have already dwelt on the absolute necessity there exists for great care on the part of both patient and physician when the cervix is to be dilated, when any cutting or burning operation is required, or when we intend to apply any curative remedy to the interior of the uterus, or to inject its cavity. Suffice it to say, that the surprising rapidity with which pelvic inflammation so frequently

* See p. 94.

supervenes from even the most trivial traumatic influence, will soon teach the young and inexperienced practitioner the necessity for caution.

In the acute stage, then, of either of the forms of pelvic inflammation we have spoken of, it may be necessary to be so far antiphlogistic as to remove blood by leeching. Of course the amount to be taken from your patient is to be regulated by her general strength, and according to the form of inflammation and variety of the disease she is suffering from. A phlegmon will require the removal of blood to a less degree than a circumscribed peritonitis. A puerperal patient, again, will probably be seldom able to bear much additional loss of blood: the sufferer from the gonorrhœal variety is usually too much weakened by the long-continued discharge which generally precedes the outbreak of the secondary affection: and the same will, in all likelihood, be true of those in whom the local peritonitis or cellulitis is only the complication of an organic disease. In the menstrual variety, on the other hand, when the flow has been suddenly checked, and in most of the traumatic forms, leeching, even to the withdrawal of a considerable amount of blood, will not only prove useful, but may even be imperatively required. The rules and method of leeching have been dwelt on at so great length by many of the French authors, that the variety of directions given, and the number of precautions they say are necessary, might almost drive an ordinary practitioner to distraction, if he were to attempt to pay any attention to them. For my own part, I have never yet seen a case in which I thought it essential to take the blood from the cervix direct. I have, in every case I employed leeches, attained all that I desired by allowing them to fasten round the anus; and the communication between the hæmorrhoidal veins and those of the uterus and its appendages, is so free that we may almost consider this to be direct leeching. My great objection to the introduction of a speculum, of such a size and shape as to be suitable for leeching, into a hot and irritable

vagina, is that it must press injuriously in nearly every case against an inflammatory mass, an immobile uterus, or a displaced and tender ovary.

But if the local detraction of blood is required more in pelvic peritonitis than in the simpler form of pelvic inflammation, the use of opiates may be said to be even still more imperatively demanded in the former. The continued and severe pain, the obstinate vomiting, and the early exhaustion of the patient in consequence of the retching and straining, force us to keep our patients as much as possible under the influence of narcotics, and the most powerful of those remedies are usually necessitated. Opium and morphia suppositories of the strength of a fourth of a grain and upwards, repeated at frequent intervals, and the subcutaneous injection of strong solutions of morphia,* are generally the best forms for the administration of that valuable narcotic, as the nausea and sickness often prevent us giving anything but ice by the mouth. Other anodynes, such as cannabis Indica, conium, hyoscyamus, etc., may be used, and sometimes are required, but I must confess to having little confidence in their pain-destroying properties in such acute affections as inflammation of the pelvic peritoneum, although they may perhaps be found useful in the few cases in which you ascertain that there is a distinct idiosyncrasy on the part of your patient against the use of opium.

Besides these general and local means, we must have recourse at the commencement of all such cases to constant poulticing or stuping, and it is generally advisable to add to the poultice some soothing tincture, liniment, or oil. The French, in cases in which there is much tympanitis, recommend the application of ice-bags over the abdomen; but most English physicians declare their prepossession in favour of the older method of using heat and moisture

* In the last case I began with a quarter of a grain of the bimeconate of morphia in solution, but had rapidly to increase it first to one half, then to a whole grain, and finally to a grain and a-half, twice-a-day, to keep the intense pain in subjection.

combined, in every case of peritoneal inflammation. From mercury, in any shape or form, I have never seen any benefit accrue; but to those who are wedded to the notion that its action is necessary for the resolution of a serous inflammation, it has been suggested* that the best form in which it could be given, would be that of medicated pessary or suppository. In both ways, a slight action on the gums can very soon be obtained. In the first week or ten days of the acute stage, very little good is to be had from acting on the bowels by purgatives, though occasionally we require, by means of some simple enema, thrown up through a long tube,† to remove any hard or impacted fæcal masses, which, by accumulating in the sigmoid flexure or rectum, may be pressing injuriously on the tender swelling in the pelvis, and thus aggravating the sufferer's pain.

The vomiting is another of those distressing symptoms, which frequently requires our interference, and I must confess, very often baffles all the ingenuity and skill of the medical attendant, occasionally proving even so exhausting as to destroy the patient. Small doses of dilute hydrocyanic acid, bismuth in powder or solution, oxalate of cerium, charcoal, cherry laurel, or lime water, small doses of opium, blisters to the epigastrium, the spinal ice-bag,‡ and the constant use of ice may all be tried in rapid succession, and each successive remedy prove as futile as the one preceding. Sometimes we are obliged to abstain from giving food or medicine of any kind by the mouth and to trust to nutritive enemata solely, though in a case of pelvic peritonitis the necessity which should induce us to do so must be dire indeed, as one of the main objects of our treatment ought to be to keep the lower bowel as quiet as possible.

* Sir J. Y. Simpson's Lectures on Diseases of Women.—Medical Times, 1859-60

† See Aran, *op. cit.*, p. 737.

‡ In two very severe cases, I imagined that the spinal ice-bag checked the vomiting, when all other remedies had failed, but no opinion can be founded on two cases.

When the active symptoms have subsided, and the complaint has passed into the chronic stage, the treatment will vary according to whether we have an abscess formed or not; but to enter even superficially into a discussion of either point would lead me too far. In the two cases to which I have just alluded, and in which I evacuated the pus, the success justified the treatment in my eyes, whatever it might do in those of such an opponent of active interference as Aran. I would only call attention to what others I believe have also stated, which is, that an abscess may exist without our being aware of the fact. I have occasionally been startled by the appearance of pus from the bowel or bladder, when I had not even the faintest suspicion that suppuration was going on in the pelvis, Nonat, it is true, maintains that the occurrence of suppuration is almost invariably due to the neglect of intelligent treatment on the part of the practitioner; but as his intelligent treatment consists in repeated venesections, I am afraid that most of our brethren in this country are likely to remain great sinners in his estimation. Perhaps I may be allowed to point out, however, that we may sometimes prevent the disease becoming chronic by carefully guarding our patient from imprudent acts until at least the first menstrual period has passed, and thus warding off those exacerbations of the original evil, the *redoublements inflammatoires*, which are so trying to the patience both of physician and patient.

In quitting the treatment of those important affections, I cannot help quoting a passage from Bernutz:—"I ought to add," he says, "that I have found conium a most valuable special narcotic to the genital organs. I give it so as just to produce slight derangement, and a kind of hallucination." I have never tried it, but an observation of the kind from so attentive a clinical physician as Bernutz is very valuable.*

* Compelled by the state of my health to winter in the south of England, I have been quite unable to get at many of the books, periodicals, or journals alluded to, and have been obliged to trust to my memory to a much greater extent than desirable. I must therefore crave the indulgence of my readers for any inaccuracy in reference or quotation they may notice.

Dr Young mentioned a case in which inflammation had been set up by the introduction of a tangle-tent. The tent was left in for seven hours, and great pain was caused, peritonitis came on, and, after a severe illness, the patient recovered.

Dr Burn said that intrauterine pessaries and sponge tents sometimes caused much mischief.

Dr Keiller said he approved of the old names of pelvic peritonitis and cellulitis, which he believed were more expressive than the new ones introduced by Dr Duncan. He had a case some time ago, where, after dilating the cervix by a sponge tent, peritonitis came on and proved fatal. Dr Aitken speaks of abscesses occurring external to the peritoneum. Now, he was of opinion that cellulitis ought to be described as of two varieties, pelvic and abdominal, as pointed out by Dr Bell. He had at present under his care a case in which the pus was poured into the bladder and discharged with the urine. Another case he had met with in the Infirmary, which proved fatal on account of the matter bursting into the peritoneal cavity. It is best when the matter is evacuated by the vagina.

One of the instruments shown by Dr Cordes as Chassagny's dilator was very like one he had himself shown to the Society some years ago for introducing tangle or sponge tents into the neck of the womb. It did not answer the purpose, however, but in fact it was not required. The india-rubber bag attached to Chassagny's instrument is too small.

Dr Bell was of opinion that it was best to distend the india-rubber bag with air, for if water were used the weight might cause the bag to slip out of its place.

SERIES OF PHOTOGRAPHS OF DEFORMITIES.

EXHIBITED BY G. DICKSON, M.D

APRIL 13, 1870.

DR DICKSON exhibited a series of photographs of supernumerary digits, a description of which will be found in a paper by Professor Struthers on that subject.

CASE OF MENSTRUATION DURING PREGNANCY.

By A. H. Balfour, M.D., Portobello.

April 13, 1870.

Mrs W., aged 46 years, has had twelve children, and all were natural labours. She was quite well up to May 1869, but the discharge was always very great, and continued more than three days. In July the breasts were very large and hard, as also the glands in the axillæ, and there was so much pain in attempting to work that she had to give it up for some weeks. After this she altered every Saturday for about six weeks; which led her and others to suppose that a change of life was coming on, and which was farther confirmed by her ceasing to menstruate for three months. It then returned on the 26th of each month with considerable discharge, lasting for three days, and this continued until the birth of a female child on the 25th of February 1870, after a natural labour, with the placenta attached to near the fundus of the uterus. The child weighed ten and a-half pounds, and was quite healthy. The mother made a good recovery.

ON THE PRODUCTION OF PRESENTATION OF THE FACE.

By J. Matthews Duncan, M.D.

April 13, 1870.

It has always appeared to me to be beyond reasonable doubt that the immense majority of face cases are the result of derangement of the usual mechanism of the first part of the progress of the fœtal head through the pelvis; or, that presentation of the face is the result of a displacement of the vertex backwards as regards the child, the extension of the head being produced at or near the brim of the pelvis by the propelling powers of labour.

My object in the present paper is to show the mechanism by which this change from a vertex to a face presentation is probably effected.

As has already been hinted at, all cases of face presentation do not demand this kind of explanation. Instances are recorded, for example, where the extreme extension of the head, existing in an ordinary face case during labour, has been observed before labour commenced. To this extraordinary kind of face presentation, and to some others allied to it, I make now no reference whatever, confining myself to the mechanism of the production of face presentation in ordinary circumstances.

Quite recently, a remarkably interesting and valuable contribution has been made to our knowledge of face cases by the pamphlet of Dr Hecker of Munich, entitled "Ueber die Schaedelform bei Gesichtslagen," published at Berlin in 1869. In this work Hecker clearly shows that a great majority of children born with the face presenting have the dolichocephalous form; the height of the skull is small, it is prominent posteriorly, and has narrow and but slightly bulging parietal bones. These conditions of the cranium he demonstrates, so far as his collection of measurements justifies him, to be not the result of the labour, but to be original and permanent states of the heads of children born in this peculiar manner.

This discovery would naturally lead to the supposition that the face presentation is a consequence of the dolichocephalous condition. Hecker does not expressly say that it does, but the direction of his thoughts may be fairly guessed at by regarding the importance which he attaches to the comparative increase of length of the posterior cranial lever-arm, and the nicety with which he has investigated this point. Hecker has omitted to mention another point in the dolichocephalous structure, which, though dependent on the increase of length of the posterior cranial lever-arm, is not identical with it, namely, the projection of the occiput; a condition well indicated by the obtuseness of the mastoid angle or angle

formed by the junction of the squamous and lamdoidal sutures.

The production of face cases, as I am about to describe it, does not depend upon the increased comparative length of the posterior cranial lever-arm, nor on the projection of the occiput, but depends upon a quite different mechanism. This I shall presently describe. But I wish here to point out the place and power of the dolichocephalous form, as introducing a condition in which the mechanism alluded to will operate at extraordinary advantage, and therefore will operate with extraordinary likelihood of producing the transformation. The transforming mechanism will produce the transformation with greater certainty and ease if the child is dolichocephalous than if its head is of ordinary construction.

The mere dolichocephalous condition explains nothing, for even in the dolichocephalous the comparatively long posterior cranial lever-arm is shorter than the anterior cranial lever-arm.*

In ordinary labour, with the vertex presenting, whether the occiput looks to the mother's right or to the mother's left, transformation into a face case is prevented by the greater length of the anterior cranial lever-arm. Strong pressure equably exerted on the upper surface of the cranium, as it will be if the longitudinal axis of the uterus is parallel or nearly so to the axis of the pelvic brim, will lead to flexion, not extension of the fœtal head. In other words, if there be uniform resistance to the advance of all parts of the cranium, as there will be in the condition of the uterus above named, the occiput will advance first, if the resistance is sufficient to produce flexion of the head. If, in the same circumstances, the posterior cranial lever-arm were the longer, then extension of the head

* In Hecker's eight cases of delivery under face presentation, in which he made careful measurements of the anterior and posterior cranial lever-arms, the posterior was shorter in every case than the anterior. The average difference between the two was, in fœtal heads generally, 1 centimetre. In the eight face case heads, it was ·4, ·1, ·2, ·4, ·6, 1, ·1, 1·7.—*Ueber die Schaedelform, etc.*, s. 46.

and forehead presentation would result, the first step towards face presentation, which would subsequently be produced.

The above statements may be held as true, because the relative length of the cranial lever-arms is the only disturbing element of what would otherwise secure uniform progress in unchanged conditions. In that part of the pelvis where transformation of the vertex into a face case takes place, the axis of the passages is one and unbent, except by the usual slight lateral obliquity of the uterus to the right. To the bending or curvature of the passage produced by this right lateral uterine obliquity we shall afterwards draw particular attention. The curvature of the genital passage which begins posteriorly about the middle of the third sacral vertebra, a curvature in the antero-posterior vertical plane, is an important curvature in the mechanism of parturition; but it is not important in the subject now under discussion, for the simple reason that the transformation of a vertex into a face case is ensured before the cranium affronts the commencement of this antero-posterior curvature in the vertical plane. Were this great curvature not so situated, its influence would be great. It is very great in the mechanism of the descent of the head through it under a pressure with a uniform direction. But this has no relation to presentation of the face, which, no one can doubt, is determined before this part of the genital passage is reached.

Although the great antero-posterior vertical curvature of the genital passage does not interfere in the production of face cases, there may be interference from another curvature, which is produced by the well-known usual right lateral obliquity of the uterus. This very frequent obliquity produces a curve of the genital passage at or near the brim, whose mesial line is concave to the right. No doubt, the so-called erection of the uterus during pains will tend to diminish, and, perhaps, will at last obliterate, this curve, bringing the uterine axis into the antero-posterior mesial vertical plane. But, while it is

not proved that this complete obliteration of the right lateral obliquity of the uterus takes place at the acme of a so-called uterine erection during a pain, it is certain that, at the beginning and ending of a pain, there is, in the sitting or standing position, or in the dorsal decubitus, a right lateral obliquity of the uterus, and consequent curvature of the genital passage, whose influence in producing the transformation of a vertex into a face case must be carefully considered.

To show how frequent is the deflection of the uterus to the right, I shall quote some figures from the work of MM. Dubois and Pajot referring to this point:—"Of 100 pregnant women," say they, "taken without selection, and examined at the Hospice de la Maternité, at the commencement of the ninth month of pregnancy, 76 presented a very evident right lateral obliquity; 4 a left lateral obliquity; 20 an anterior obliquity without appreciable lateral inclination." *

Now, in pushing a plastic body, such as a fœtus through a curved tube, such as the pelvis, and supposing that the resistance is equal at all parts of the tube, that part will be pushed with greatest advantage which is nearest to the line of the propelling force, and it will advance first. In the example referred to, the parts nearest the concave side of the passage will advance first.

We now proceed to apply this general statement to the subjects under discussion.

In the great majority of cases of right lateral obliquity of the uterus, the occiput of the fœtus occupies the concave side of the passage of the genital canal at or near the pelvic brim. If the head meets with much resistance, the occiput will tend to advance first, from the comparative shortness of the posterior cranial lever-arm, and from its position nearer the concavity of the passage, and therefore nearer the line of the propelling force, which must incline towards the left in consequence of the deflection of the uterus. The vertex presentation will be maintained.

* Traité Complet de l'Art des Accouchements, tome i. livr. 2me, p. 381.

In a certain proportion of cases, nearly 1 in 3½, the foetal occiput looks to the right side of the passage, to the convexity of the canal, and the forehead to the left side or concavity of the canal. The propelling power, *per se*, will therefore tend to make the forehead descend, and bring on a forehead presentation in anticipation of a fully-formed face case. This tendency to push down the forehead instead of the occiput will be, in the majority of cases, counteracted by the agency of the more efficient resistance by the soft parts to its advance in consequence of the greater comparative length of anterior cranial lever-arm. But, in a case of exaggerated right lateral obliquity, this resistance may be overcome, and the forehead may descend. In every case in which the dolichocephalous form exists, the resistance from the comparative length of the anterior cranial lever-arm will be diminished, or, in some cases, even almost annihilated, and the forehead be invited to descend first, the head becoming extended.

Now, it cannot fail to strike the intelligent student, as being more than a coincidence, that while right lateral uterine obliquity is the ordinary one, face cases are comparatively much more frequent in the second position—that is, with the occiput looking to the right or the convexity of the canal, before the transformation; and that the children are generally dolichocephalous. In other words, the conditions which can be shown, *a priori*, to be likely to lead to the transformation of a vertex into a face case are exactly the conditions which are found, not indeed invariably, but to prevail.

"The numbers," says Professor Hecker, "which I have previously given for the proportion of the first to the second face position, 22 : 21, are now changed, so that now 28 first and 21 second positions are noted; the proportion of the latter to the former, therefore, is expressed as 1 : 1·3, and if I compare with this my former calculations in reference to first and second vertex positions, which yielded a proportion of 1 : 2·3, then I must justify Winckel when he judges from his statistics,

that the second position of the face in proportion to the first, occurs oftener than the second position of the cranium to the first; that, therefore, *ceteris paribus*, a face position is easier produced when the back of the child lies to the right than when it lies to the left."—(S. 15).

It appears to me, then, to be highly probable that the chief cause of face cases is obliquity of the uterus in any direction, ensuring a curvature of the genital canal at the brim of the pelvis; that this cause operates when the forehead of the child is placed near the concavity of the curved canal or nearest the line of the propelling force; and that the dolichocephalous form will greatly favour the transformation under these circumstances of a vertex into a face case.

Before the study of face cases led me to the above-mentioned results, I, and others, used to think and speak of hitching or obstruction of the occiput on some part of the brim of the pelvis, leading to its complete arrestment, and the subsequent extension of the head, the forehead, and then the face descending. This hitching, if it ever takes place, will be evidently favoured by the projection of the occiput in the dolichocephalous form. But such ideas were admittedly very vague and unsatisfactory, for such hitching can scarcely even be conceived except in a case of deformed pelvis, and it is only a small proportion of face cases that actually occurs in complication of labour with deformity of the brim of the pelvis. Though the hitching of the occiput is a very rude and utterly insufficient explanation of the majority of cases, it may possibly occur in some of the face cases which are produced where there is deformity of the brim. I am far from saying that face presentation is so produced in cases of pelvic deformity, for examples of this are far too little known; their peculiarities have been as yet far too little studied to enable us to come to any conclusion whatever on the subject of their special causation.

Before concluding, I shall draw attention to a very remarkable circumstance, which the above proposed

theory of face cases may explain. I find that Hecker and other foreign authors ascribe to face cases a frequency far in excess of what is, I believe, observed in this country. Hecker says there is a face case in every 168. In Collins's Dublin Hospital experience, there was only 1 in 497. Now, this remarkable difference* may be explained by differences in the position of the women during labour in various countries. The almost invariable decubitus of British women on the left side may prevent the transformation of many vertex into face cases that would otherwise be so changed. I am not sufficiently informed to contrast this position of British women with the dorsal decubitus or other position of German women in like circumstances. But it appears to me to be a subject worthy of attention.

Dr Pattison said that in a thousand cases he had met with only two face presentations.

Dr Burn had met with a number of face presentations in his practice, but few comparatively. In such cases he usually found that the fœtal head was small, so that almost invariably he was able to pass his fingers over the ear, and thus rectify the position. Face presentations are difficult to diagnose till the head is well into the pelvis.

Dr Gordon asked Mr Duncan whether it was possible to change the position after it was diagnosed. He had met with only one case.

Dr Duncan said he had no great experience in changing the position.

Dr Bell called attention to the fact that Naegelé believed that face presentations were most frequent in the first position.

Dr Keiller believed that face cases were more common in multiparous than in primiparous women; the abdominal walls being more lax in the former allowed the

* "Thus," says Churchill (Theory and Practice of Midwifery, 4th ed., p. 409), "in British practice, out of 141,259 cases, there were 567 face presentations, or 1 in 249; among the French, 50,141 cases, and 189 face presentations, or about 1 in 265½; and among the Germans, 69,417 cases, and 411 face presentations, or about 1 in 169½; the whole giving 1167 face presentations in 260,817 cases, or about 1 in 223½ cases."

uterus to roll about. In his own experience face cases were rare.

Dr Milne used to think that a hitching of the occiput was the cause of brow presentations, but further experience had failed to confirm this notion.

ON THE SYNCLITIC MOTION OF THE FŒTAL HEAD IN THE MECHANISM OF PARTURITION.

By J. Matthews Duncan, M.D.

April 27, 1870.

In an elaborate, ingenious, and valuable work recently produced at Berlin, by Dr W. L. Kueneke, and entitled, "Die vier Factoren der Geburt. Grundzuege einer Physik der Geburt," several alleged phenomena in the mechanism of labour are for the first time described. Of these, the synclitic movement of the fœtal head in its course through the true pelvis appears to me to be one of the most important. It is alleged to be followed by a quite opposite kind of movement of the head as it begins to distend the perineum, called the enclitic movement. It is my object at present to investigate this so-called synclitic movement, that it may be confirmed and accepted, if found to be true; and that it may be rejected, if found to be false, and the older views confirmed, elucidated, and explained. All this is particularly necessary, because the character of the progress of the fœtal head in the true pelvis, whether synclitic or oblique, has never attracted the attention which it deserves. In the sequel, indeed, I hope to satisfy the student that Kueneke himself, while fully alive to the importance of the subject, has omitted to bring to bear upon its discussion one of the chief agencies influencing it, and some collateral results throwing light upon it.

In order to explain, first of all, what is meant by the synclitic movement of the fœtal head in its progress

through the pelvic cavity, I go back to refer to some points in its progress through the first part of the same bony canal. It is well known* that Naegelé described the fœtal head as entering the brim of the pelvis obliquely as regards the plane of the brim,—that is, with its vertical axis not in the direction of the axis of the brim; with the plane of a horizontal section of the head not in the plane of the brim, but with the edge of this imaginary horizontal plane which looks anteriorly advancing first, with a lateral obliquity, in a position involving left lateral flexion of the fœtal head. Adapting Kueneke's nomenclature to this part of the progress of the fœtal head, we might say that Naegelé described the fœtal head as entering the pelvic brim, not synclitically, but obliquely or antisynclitically. All recent authors, so far as I know, even including Kueneke, who have carefully studied this point, have differed from Naegelé, and described the fœtal head as entering the brim synclitically, not obliquely, with its horizontal plane in the plane of the brim, the plane of its vertex synclitic with the plane of the brim. It may be held as settled, that in the first part of its course through the true pelvis, the fœtal head does advance synclitically.

Very few authors have paid special attention to the exact condition of the fœtal head in this respect in its course through the pelvis. Those who have described it are, with the exception of Kueneke, so far as I know, of opinion that its progress is oblique, at least in the latter part of its course. Kueneke asserts that it is synclitic. It is generally held that when the fœtal head begins to leave the direction of the axis of the brim—begins to advance forwards—it advances obliquely, not with a point in the mesial line leading, but with a point in a parietal bone leading. Kueneke's doctrine implies that a mesial point always leads, a point in the sagittal suture.

* In the whole of this discussion, I shall assume that the advancing fœtal head is in the first position, the occiput directed towards the mother's left. It is scarcely necessary to add, that the kind of obliquity here considered is what Kueneke aptly designates the Naegelé obliquity. The Roederer obliquity and the Solayres obliquity are not the subjects of this paper.

If we define as the presenting point* that point on the surface of the child's head through which the axis of the developed pelvic canal passes; then, according to Kueneke's view, the presenting point is always in the mesial line, always in the sagittal suture, and must be so. According to most authors, the presenting point is, at least after the head begins to advance forwards, situated on the anterior parietal bone. If, indeed, the head becomes rotated so completely as to bring the sagittal suture into a direct antero-posterior position, then the presenting point will be, as in the passage of the brim, in the mesial line, in the sagittal suture, but only then. Here is a great difference between Kueneke and other authors. Kueneke describes the course of the head through the true pelvis as being synclitic; authors generally describe it as being oblique in the latter half of this progress.

The difference between Kueneke and others may be farther elucidated by stating it as follows:—Kueneke's synclitic motion of the fœtal head keeps the point of intersection of the antero-posterior and transverse diameters of that plane of the true pelvis through which the fœtal head is passing always in the sagittal suture. Other authors hold that the intersection of these diameters is in the anterior parietal bone, in the latter part of this course through the pelvis.

"Let us," says Kueneke,† "regard this canal of the pelvic cavity, curved as it is towards the symphysis, as divided by any chosen number of transverse-sectional planes, which the head has to go through, one after another, then, as these planes diverge in the sacral direction, the head makes in every succeeding plane a turning movement, whereby it is kept parallel to each plane as it reaches it, so that at length arrived at the last plane, close to the plane of the outlet, it stands parallel to this also." This synclitic movement is said by Kueneke to have the symphysis for its fulcrum, the transverse diameter of the

* Duncan's Researches in Obstetrics, p. 369.

† Vier Factoren der Geburt, s. 36.

head representing a single-armed lever moving on the symphysis as a fulcrum through an arc of 30 degrees, a figure which is said to be the measure of the difference between the inclination of the brim or inlet, and that of the outlet as defined by Kueneke, or of a line running in sagittal direction through the top of the pubic arch and the point of the sacrum (not of the coccyx).

"To attribute," says Kueneke, p. 38, "to the symphysis the importance of a point of support for the revolving lever, is, in my judgment, perfectly well founded: but withal, the mechanism of the synclitic movement is still by no means sufficiently elucidated. For there is still reserved the question, how at last the symphysis comes to be in a position to offer a resistance to the driving power; a question which has as yet not been once proposed, far less answered. I shall attempt to make out a solution of this not very simple mechanism.

"We know that the driving power falls at a right angle on the plane of the inlet, that its axis bisects its sagittal diameter, and passing through the cavity of the pelvis permeates the coccyx, and is inclined to the horizon at an angle of 30 degrees. Now, the matter would be very simple, if it could be shown that the symphysis was so situated as to converge towards this unchangeable direction of the driving power. If, to show this, there be drawn at the symphysis a line parallel to this direction, then there is at length found to be no convergence, not even parallelism, but, on the contrary, a divergence to about 10 degrees, of the symphysis, which is inclined at an angle of 40 degrees. Here, then, can therefore be no talk of resistance of the symphysis, and consequently none of synclitic movement. Much rather would this one-sided treatment of the relations establish the persistence of the orthophoric movement all through the pelvic cavity, and justify those who adhere to the theory of obliquity of the cranium in the pelvic cavity.

"As the symphysis does not, without some further condition, offer the expected resistance, we must look about

for something else which may be fitted to afford to the symphysis the postulated peculiarity. This I find in the posterior pelvic wall. If we draw at the posterior wall of the pelvis, from the promontory onwards, a parallel to the direction of the driving power, it is found to coincide with the anterior surface of the two upper bones of the sacrum, passes diagonally through the third vertebra, and then runs behind the sacrum. There is thus from above to this point a lessening degree of friction between the advancing head and the two pelvic walls implicated, which compresses the skull in the direction of its transverse diameters; but as yet no one-sided resistance, which might produce a revolving. Evidently rather the friction begins first at the third sacral vertebra, and increases from hence downwards more and more. This friction is, nevertheless, still not in a position to produce a distinct resistance, which in case of its being in excess would act as a fulcrum; but the inclined planes of the excavated vertebræ exert, as the cranium glides over them, a pressure against the symphysis. This result has already been expressed by Roederer in these words 'Vis sane composita oritur, ex dolorum impetu, qui in directione axeos pelvis agit, et plani inclinati et mobilis (sc. posterioris vaginae parietis), cujus directio ad anteriora vergit, renixu.' Here, now, there would be produced a resistance, even supposing the symphysis formed a convexity corresponding to the sacral excavation. So much the more must this resistance come into play when the direction of the symphysis, in spite of its divergence from the direction of the driving power, has a not insignificant convergence with the direction of the lower part of the sacrum; through which last, as has been already shown, the direction of progress is in a corresponding manner inclined forwards; and this resistance, which, at the same time, is not strong enough to arrest the advance, forms the fulcrum round which, under a constantly diminishing progression from above downwards, the implicated transverse section of the cranium revolves as a single-armed lever, and so keeps

in parallelism with each successive transverse-sectional plane of the pelvis into which it is brought in consequence of its advance. This is the synclitic movement."

I have thus given Kueneke's theory in a nearly literal translation of his own words. To what I have quoted, Kueneke adds very little further. Only, I may add, that he expresses his belief that, through the whole of the pelvic cavity, the head advances synclitically.

Before proceeding to discuss the statements and theory of Kueneke, I shall give a brief account of what I (in common, I believe, with many obstetricians) hold to be the facts in regard to this point in the position of the head as it passes through the cavity of the pelvis, from the brim to the outlet.

The fœtal cranium enters the brim or inlet directly or synclitically, that is, without lateral obliquity or lateral flexion. The presenting point is in the saggital suture. The transverse and antero-posterior diameters of the brim intersect in the saggital suture. Regarding only the matter now in question, the fœtal head advances, in unchanged relations, in the direction of the axis of the brim till it is arrested by the sacrum and the floor of the pelvis. In this way it often advances unchanged even till the vertex is nearly pressing on the coccyx. Up to this point its progress is direct, unchanged, because it has met with no special resistance; the vertical axis of the head is still parallel to the axis of the brim. So long as the head was passing through the upper half of the pelvic cavity, the plane of the vertex was synclitic with the successive parallel planes of that part of the pelvis. But there was no new mechanism to produce this synclitism. The head simply advanced in unchanged position thus far. As I have already said, it generally advances still farther in the same direction, and till it meets the resistance of the lower half of the sacrum and floor of the pelvis, maintaining its original position towards the axis of the brim; the axis of the brim still, when continued downwards, traversing the sagittal suture. But as soon as it

so advances beyond the middle of the pelvis, beyond the transverse sectional plane which passes through the middle of the third sacral vertebra, it ceases to be synclitic with the succeeding planes, and it never regains completely its original synclitism such as it had in the upper half of the pelvis. The axis of the second half of the pelvic cavity is never again (unless, indeed, the head so completely rotates as to have its saggital suture lying exactly in the antero-posterior pelvic diameter) found traversing the saggital suture, but traverses a point on the surface of the anterior parietal bone. The antero-posterior and transverse diameters of the transverse-sectional planes of the lower half of the pelvis never intersect in the saggital suture.*

In making these statements of what I believe to be the facts in nature, I do not assert that there is no tendency or progress towards synclitism in the second half of the pelvic cavity. There is certainly such tendency, and I believe there is also, as a result of the tendency, an approach to the synclitic state. But the synclitic condition is never reached.

I shall now proceed to give the evidence upon which my statements as to the absence of synclitism in the second part of the progress of the fœtal head through the pelvic cavity (and, indeed, in its still further progress also are founded.) The great peculiarity of Kueneke's description of this matter lies in his assertion and attempted demonstration that there is a movement of the fœtal head keeping it synclitic as it advances. I believe it is not synclitic in the second part of its course.

I shall arrange the evidence I have to adduce under five different heads.

First. I shall appeal to observation, and in this, of course, Kueneke will join me. I may, however, describe a common condition, and assert that its truth will be generally recognised, and that it is quite inconsistent with Kueneke's views.

* For another statement of the same facts, see my "Researches in Obstetrics," p. 358.

When, during an ordinary labour,—say, for example, a first labour, in which there is plenty of time for careful observation,—a digital examination is made when the vertex has reached the plane of the outlet of the ligamentous pelvis or of the pelvic excavation, or nearly reached it; when, in addition, as often happens, the case is favourable for the purpose, from the cranium having not yet begun, or only slightly performed its regular rotatory movement on its vertical axis, it is found that the sagittal suture lying transversely, or more or less in the right oblique pelvic diameter, is far behind or posterior to the transverse diameter of the plane which the vertex or upper half of the fœtal head is occupying. The antero-posterior and transverse diameters of the plane occupied by the head do not intersect in the sagittal suture. The sagittal suture is intersected, or nearly so, by the axis of the brim prolonged downwards; it can be felt to be just above the coccyx. The anterior parietal eminence can be felt in the upper part of the pubic arch, and nearly in the same pelvic plane as the sagittal suture. The sagittal suture is far behind the transverse diameter of this part of the pelvis. The axis of this part of the pelvis traverses the anterior parietal bone, upon which, therefore, lies the presenting point. The head is not placed synclitic with the transverse-sectional plane of this lower part of the pelvic cavity. The anterior side of the head is in advance of the posterior.

I choose the position of the head above described for two reasons, which render it suitable for testing and for disproving Kueneke's views; for at this stage of progress we still have the coccyx and other bony landmarks to indicate whereabout the axis of the developed genital canal lies. At a more advanced stage these landmarks are absent, and the ascertainment of the position of the axis is rendered more difficult, on account of the head in its progress developing, and at the same time occupying the passage, and thus introducing difficulty. At this stage of progress, also, there is often very little rotation

effected, and the occurrence of rotation renders it difficult to judge regarding synclitism. Continued synclitism is conceivable during and after rotation, but Kueneke does not say it then exists.

Before leaving this part of the subject, I must make Kueneke's own testimony available against his synclitic doctrine. Kueneke describes very clearly the position of the sagittal suture after the head has undergone its rotation. This description he ends as follows :—"If we trace the sagittal diameter of the outlet upon the head as it lies in this position, its course will be, from the top of the pubic arch, about two centimètres distant from the sagittal point of the small fontanelle, starting from the right limb of the occipital suture, coming gradually to convergence with the saggital suture near the forehead, intersecting it at its ending in the great fontanelle, passing over this last to the front of the left frontal bone, and on to its tuber, and further somewhat laterally to the left orbit." Now, this description by Kueneke is the description of a head which has certainly rotated, but which has not become synclitic. Kueneke does not notice this circumstance, and, of course, does not account for it. Were the head synclitic before rotation, it should be synclitic during it. But while the head is not synclitic, it is exactly situated as my views would lead one to expect. It is advancing with the anterior or right parietal bone, leading with the posterior or left parietal bone, on a higher transverse-sectional plane of the pelvis than that occupied by corresponding parts of the right. Before it rotated it was not synclitic, and Kueneke's descriptions are erroneous. After it has rotated, it still is not synclitic or in the analogous state of its new position, and this accords with Kueneke's description just given, but is subversive of Kueneke's views;* for the axis of the genital passage traverses the right parietal bone. The horizontal plane of the child's head is not synclitic, for the whole sagittal suture is to the left of the mesial line. The axis of the

* Kueneke's enclitic movement is supposed not as yet to have commenced.

pelvis does not pass through the sagittal suture, but through the parietal bone on the right side of the sagittal suture, just as it did before rotation commenced. It is thus evident that Kueneke has himself described the head in a position which is inconsistent with his own synclitic doctrine.

Second. Kueneke describes an enclitic movement of the fœtal head succeeding to the synclitic movement. While I do not admit the accuracy of Kueneke's description of this enclitic movement, so far as it is novel, yet, there is no doubt that it is, as Kueneke says, truly antisynclitic. It is opposed to the whole synclitic theory. The enclitic movement is observed after the rotation of the head, and consists essentially in a flexion movement. The synclitic movement involves comparatively rapid descent of the parts of the head lying near the posterior wall of the genital canal. The enclitic movement implies retardation or lagging behind of the parts situated posteriorly. Kueneke will, of course, reply to our statements, and to a great extent truly, that synclitism and enclitism are different conditions not justly to be compared. To this we reply, that if enclitic movement takes place, synclitic movement is thereby rendered improbable; that the same circumstances which produce the enclitic or antisynclitic state after rotation of the head prevent the synclitic state before the rotation. According to our view, the so-called enclitic state is merely a continuation and increase of the antisynclitic condition which previously existed.

Third. I have already, near the commencement of this paper, given, in a translation of a passage from Kueneke's work, that author's own description of the mechanism by which the so-called synclitic movement is supposed to be produced. To that description I object on two grounds, —namely, that it is scientifically incorrect, and that it omits the chief agency that Kueneke should have invoked to support his own doctrine.

Kueneke's description of the supposed mechanism of synclitism is scientifically incorrect; for it assumes that

the reaction is greater than the action. Kueneke says the posterior wall of the pelvic passage pressing on the adjacent left side of the head fixes, or nearly fixes, the remote right side of the head upon the symphysis, so that this right extremity of the transverse cranial diameter becomes the nearly fixed end of a lever represented by this transverse diameter. Now, supposing no other force acting, these pressures on both ends of this transverse diameter of the head must be equal and opposite, and in such a case there will be no tendency of the one extremity of this diameter to be more retarded by friction than the other. But there is another force acting, namely, the propelling power, whose direction is such as inevitably to make the pressure on the posterior wall or on the left extremity of the transverse cranial diameter, and therefore the friction there, much greater than on the anterior pelvic wall.

But Kueneke's statement of the conditions and results is farther incorrect. If the head, arrived in the lower half of the pelvic cavity, is pressed forwards against the pubes, it is also pressed downwards by the driving or propelling power, and the resultant will not be a pressure against the symphysis leading to fixation by friction of the part of the fœtal head adjacent to the symphysis, but a tendency of the whole head to advance in the direction of the axis of the pelvis, or nearly so. Though we make this statement in order to show the error of Kueneke, we wish it to be understood, that we do not adopt the mechanism implied in our exposition as the best way of reaching a true view of the advance of the head.

That there arises from the mechanism a direct tendency to the production of the synclitic condition, there can be no doubt. This is demonstrated in the following manner:— In the lower half of the pelvic cavity, the genital canal begins to be curved forwards in the antero-posterior vertical plane. In consequence of this, the posterior part of the passage, or the concavity of the canal, is always nearer the line of the propelling force than the anterior part or convexity of the canal. The posterior parts of a body

traversing the canal under these conditions will be acted on with more force than the anterior. Though Kueneke's alleged mechanism does not exist, there will necessarily be a tendency to the synclitic movement of Kueneke.

But there are two elements of this mechanism which prevent the synclitic movement. One of these is the circumstance that the fœtus is not a simple plastic mass, subject, without disturbance, to the conditions stated in the preceding paragraph. This plastic mass is furnished with a column of bones passing through it, which forms at least a main vehicle of the propelling pressure. So long as the fœtal head lies transversely, the statement of the preceding paragraph will be applicable, if there be no modifying circumstances. But the fœtal head generally has a Solayres diagonal position in this situation, and then the column of bones conveys the chief part of the force to the half of the head nearer the anterior wall of the genital canal, and the synclitic tendency is so far obviated. The increase of pressure on the half of the fœtal head nearest the pubes, in consequence of the Solayres obliquity, will not be great; but there is another reason why the synclitic movement does not take place.

The posterior half of the genital canal is the half where there is by far the greatest resistance to the progress of the head. It would not be very misleading to assert that, in that part of the course of the fœtal head, where synclitic movement is said to take place, the resistance is almost entirely in the posterior or concave side of the genital canal. In the middle, anteriorly, no stucture has to be developed. There is only a minimum of resistance from friction. On either side, departing from the middle of the anterior wall of the passage, the resistance increases till the posterior wall is reached. There the development of the passage is greatest, and this development is the source of the resistance.

"On the other hand, however," says Schroeder,* "the resistances which the head experiences at the posterior

* Schwangerschaft Geburt und Wochenbett, s. 45.

and anterior pelvic wall are very different. At the entrance of the head into the small pelvis, the anterior and posterior pelvic walls run nearly parallel; the pelvis here may be regarded as a uniform cylinder, into which the head is driven. It is evident that, through such a cylinder, the head must pass in the same position in which it enters it; and this is what the head does so long as the plevis answers to this description. But as soon as the head has completely entered it, the disimilarity of the anterior and posterior pelvic walls makes itself remarked. At the posterior pelvic wall, the resistance which the head experiences increases; at the anterior it diminishes." It is this increasing resistance of the posterior pelvic wall which prevents the synclitic movement of Kueneke, just as the same resistance produces or maintains the subsequent alleged flexion-movement, or enclitic condition, as Kueneke designates it.

Fourth. There are some of the changes in the shape of the child's head—the result of the forces to which it is, during labour, subjected — which throw light on the questions discussed in this paper. Those to which I shall specially refer are the overlapping of one parietal bone over the other, and the shears, which have recently been described by Stadtfeld, Welker, and Dohrn—especially by the last named author.

It might be expected that I should, with this view, refer to the caput succedaneum, but a little reflection will show that it can give no aid. The caput succedaneum* can only show what part of the fœtal head was, during labour, not subjected to uterine pressure; it cannot show what was the presenting point, or where, on the surface of the head, the antero-posterior and tranverse diameters of the passage intersected. The caput succedaneum shows what part of the fœtal head presented to or affronted the lumen of the as yet undeveloped part of the genital passage. If the axis of the undeveloped part of the genital passage (as at the brim of the pelvis) nearly

* Researches in Obstetrics, p. 364.

corresponds with the axis of the pelvis or fully-developed genital canal, then the caput succedaneum will be at the presenting point. But if, as is the case in the second half of the progress of the fœtal head through the pelvis, the axis of the as yet undeveloped part of the genital passage does not correspond with the axis of the fully developed pelvic canal, then the caput succedaneum will have no relation to the presenting point, or that part of the surface of the head in the axis of the pelvis; but will show where was the axis of the undeveloped part of the passage.

The equitation or overlapping of one parietal bone by another is a subject which has, as yet, been insufficiently studied. In particular, sufficient observations have not, so far as I am aware, been made as to the comparative frequency with which the right overlaps the left, and *vice versa*. But it might, I think, be expected that, during labour, the left or posterior should overlap the right or anterior, if Kueneke's synclitic movement took place, for then the left or posterior would be pushed most downwards in the pelvis; while a compressing force acting laterally on the head would cause equitation: the left would overlap the right. But I believe it is generally the reverse: the right or anterior rides over the left or posterior. The overlapping of the left or posterior by the right or anterior is, however, just what would be expected in labour under the conditions which I have described, and attempted to prove to be the ordinary or natural ones.*

Fifth. The last evidence of the directions of the pressures to which the fœtal head is subjected is the shearing†

* "For ordinary," says Schroeder, *op. cit.* s. 103, "the parietal bone lying posteriorly is shoved under the anterior; but numerous exceptions to this occur even in the quite normal course of labour; especially in the first position of the head, the right parietal bone is not rarely shoved under the left; but, truly, in a normal pelvis there most frequently takes place no evident overlapping. In the pelvis, contracted in the conjugate diameter, with or without a less degree of general contraction, the anterior parietal bone is at least as frequently shoved under the posterior; while, in the equably uniformly contracted pelvis, the posterior parietal bone appears to be almost invariably overlapped by the other."

† Thomson and Tait's Natural Philosophy, s. 171.

(*verschiebung*), described by Dohrn.* I shall give the account of it in Dohrn's own words, merely premising that we do not entirely assent to all that he says:—

"It is," says he, "a known fact, that the skull is usually placed obliquely in the pelvis in such a way, that the side of the head lying anteriorly is situated more deeply than that turned posteriorly. It is also known that the side of the head directed towards the back is usually flattened, that directed forwards more strongly curved out. Both are consequences of the more powerful pressure which the head undergoes from the posterior wall of the pelvis.

"It is also this more powerful pressure of the posterior wall of the pelvis which produces the lateral shear of the cranium.

"If you look from above on a child's head which has been born in a cranial presentation, shortly after birth you will almost always find that it is oblique. This depends partly on the position of the caput succedaneum being mostly on a side; partly, however, it depends on a lateral shear of the two halves of the skull. This last you can easily make out, if you remark the position of the tubera parietalia. You will then find that the one stands much farther forwards than the other.

"The pressure of the posterior pelvic wall brings about this shear in the following way:—If I push downwards against the promontorium a fœtal cranium whose occiput lies deep down, its side directed to the promontory comes to be shoved forwards towards the coronal suture.

"If, on the other hand, I place the sinciput deep down, and now shove the cranium downwards on the promontory, then its side directed backwards towards the promontory is shoved towards the occiput. If I let the vertex advance first, and place the great and little fontanelle at an equal height, then there comes to take place a shear corresponding to the deep position of the sinciput, and always to the greater extent the more the head is in-

* Monatsschrift f. Geburtsk, bd. xxiv., s. 418, 1864.

clined towards the forehead, the more directly from above you look at it, the less it appears to be direct.*

"This is the general law which regulates the degree and direction of the lateral shear. It is self-evident that it will be influenced by the energy of the pains, the size of the child's head, and the structure of the cranial bones; it is sufficient for me to point these out.

"Let us inquire now how far the law is confirmed by experience. I have for this purpose measured very many children's heads, and have noted carefully the appearances in the last forty which have come before me in normal pelves. During the first hours after birth, I placed a small cyrtometer horizontally around the greatest periphery of the skull, noticed at which links of the chain the frontal suture, the tubera parietalia, the occipital protuberance stood, and marked out the form of the cyrtometer on a paper divided into squares. In this way I found among the forty cases a lateral shear thirty-eight times; and it always happened that, where the half of the head turned towards the promontory had undergone a shear, pushing it forwards, there was during labour deep position of the occiput; where the reverse was the case, deep position of the sinciput was noted.

"The extent of this lateral shear in the first position of the cranium was on an average 5·3 millimètres; in the second position, where the occiput is originally more frequently directed backwards, and in consequence the pressure of the posterior wall comes more strongly into play, on an average 6·6 millimètres. At this point I must remark, that the different position of the tubera parietalia does not depend simply and alone on a shear of a parietal bone *in toto*, but to a small extent must be referred also to a change of place of the tuber in the bone itself.

"In connexion with the frequency and the degree of the lateral shear, it may appear striking that it has not earlier attracted notice. This may well arise from the circumstance, that observers have allowed themselves to

* The translator is confessedly at a loss as to the meaning of this sentence.

be deceived by the caput succedaneum, and particularly that the head is seldom carefully examined soon after birth. This last is necessary, if you wish to make out the lateral shear, for it quickly disappears, often already some hours after birth, and much earlier than the flattening of one-half of the head in the vertical direction, which is often observed for a long time. The pressure of the brain, the elasticity of the bones and soft parts, as well as the tension of the muscles, co-operate to produce this disappearance.

"The importance which I attach to my observation is not merely scientific, but also diagnostic. If we examine a child's head shortly after birth, with reference to the mentioned influences, it will be possible for us to arrive at a retrospective conclusion as to its position during labour. The flattened side of the head is almost always that which was turned towards the promontory. If it is pushed forwards, then deep position of the occiput occurred; if it is pushed backwards, then there was deep position of the forehead. In this way it will be possible to control the results of examinations made during labour, and, in cases in which the the labour was not personally observed, to form retrospectively a judgment as to the position of the head."

Before concluding, we must make a few remarks on Dohrn's valuable observations; and we must first ask our readers not to hold us as agreeing with Dohrn in ascribing to the promontory the place in the production of shear which he gives it, except in cases of contraction of the brim of the pelvis. In a natural labour the head is subjected to no kind of powerful pressure from the promontory. It is first subjected to such influences when it has passed through the upper half of the true pelvis.

Dohrn's description involves two shears, one vertical, the other lateral, which may be regarded as components of one shear.

The vertical implies the flattening of the posterior parietal bone and the bulging of the anterior. It is the

result of the resistance offered by the posterior wall of the pelvis, which, as we have before seen, prevents the synclitic movement. The adjacent parietal is flattened, and the anterior parietal, bearing the mass of the caput succedaneum, is more than naturally bulged outwards.

The lateral shear, to which alone Dohrn attaches the name (*verschiebung*) is merely that component of the whole shear which depends upon the Solayres obliquity of the head, and the rotation effected while the shear is in course of production.

Both shears at once disprove the existence of the synclitic movement, and help to explain why it does not take place.

Dr Milne said that in his paper, to which he (Dr M.) had listened with pleasure, Dr Duncan had dealt some heavy blows at Kueneke's theory. To maintain the syncliticism which existed from the brim down to about the middle third of the sacrum, an elongation of the anterior pelvic wall would be required; for the synclitic movement would cease when parity of resistance anteriorly and posteriorly ceased. The lateral shear, and the overlapping of the parietal bones, were also adverse to this synclitic theory, as well as the almost invariable presentation met with, which traversed, not the sagittal suture, but the parietal bone.

A number of questions were put to Dr Duncan, for the purpose of further elucidating the subject.

ON A CASE OF INFANTILE SALIVATION BY A QUACK MEDICINE.

By Thomas Cairns, M.D.

April 27, 1870.

A communication, on the above subject, was made to the Society by Dr Cairns.

TEDIOUS LABOUR FROM ENLARGED BODY OF THE FŒTUS.

By James Carmichael, M.D., Burntisland.

(*Communicated by J. Matthews Duncan, M.D.*)

April 27, 1870.

At 5 A.M., on the 12th March, I was called to see Mrs M., æt. 30, in her second confinement. Former labour natural, nineteen months ago. She had been in labour seven hours before my arrival. Pains recurred every half hour or three-quarters of an hour, and were by no means strong. She had last menstruated in the beginning of August.

The hand, applied over the abdomen, detected that the uterine contractions were regular, but by no means powerful. Abdomen very large. Fœtus readily felt; much more bulky than natural, and having an unusual soft feeling.

Per vaginam.—Os uteri fully dilated; both feet of fœtus in the vagina. Pains still continue slight, but now rather more frequent (every quarter of an hour or twenty minutes). The woman has a good pulse, and is quite easy and comfortable. Traction now made by the feet; but everything quite immovable. After waiting for four hours, traction was again made, when one leg gave way, and was separated at the hip. The hand was now introduced along the body of the child; the belly was found to be directed towards the sacrum, and enormously distended and soft. The hand was with difficulty passed up as far as the neck of the child, when the head was found to be laterally flexed and pressed down upon the body, and an arm under the chin. The fœtus was of a globular form. On consultation with Dr Young, it was agreed to evacuate the contents of the belly by incision or puncture, as the only means of effecting delivery.

A perforator was accordingly passed up to nearly the

umbilicus, and pushed into the belly, when at once there was a gush of yellowish brown fluid, more or less tinged with blood, from the maternal passages. By pulling and tearing the abdominal parietes, the fluid was entirely evacuated; and, the body having collapsed, delivery was very easily effected.

The patient was under chloroform during the operation.

13*th March.*—Patient going on well; no complaint. Pulse 86; some milk in breasts. Lochia natural.

15*th.*—Doing well.

18*th.*—Out of bed, and has made an excellent recovery.

The fœtus proved to be monstrous in several respects, and will be carefully described elsewhere. But the chief points of obstetrical interest regarding it are the following:—It was a globular mass, consisting of the trunk, which had been distended with fluid. To this globular mass were attached the extremities, and on the top of it was the head, not only laterally flexed, but also laterally compressed. The chief part of the globular mass was the distended abdomen; and the chest, flattened out, formed the upper wall of the abdomen. The abdomen was distended chiefly by the expanded bladder. In consequence of the want of connection between the kidneys and bladder, subsequently ascertained by Drs Sanders and Pettigrew, it is to be regretted that the contents of the bladder were not saved for examination. When the abdomen was filled with water, its girth was found to be 2 feet. A cord put around the abdomen, from ensiform cartilage, by sides, and on to the pubes, measured 2 feet 3 inches. The capacity of the whole abdomen, as measured by the quantity of water required to fill it, was 93 ounces. The capacity of the bladder was 40 ounces. The bladder measured 9 inches from apex to base, and 5½ inches transversely.

In general respects, the fœtus was fully developed, and of usual size. It was a male.

Dr Cairns said that he had, ten days ago, met with a

similar case. Dr Keiller got the preparation. He was sent for at 6 A.M. A mid-wife was conducting the case. Head had been born for two hours, and pains ceased. There were no signs of life whatever. Applied traction, but failed. He squeezed the head off, and one arm. The abdomen was filled with two gallons of serous fluid. He ripped up the belly with scissors, and removed the child bit by bit. The result was recovery of patient. After a bottle and a-half of sherry had been administered, the woman continued to improve, without a day's illness.

Dr Thomson said that Dr Carmichael's case was very rare, and displayed able treatment.

11th MAY 1870.

DR BELL, in moving that the public business of the Society should be adjourned for a fortnight, alluded in feeling terms to the great loss the Society had sustained by the death of SIR JAMES YOUNG SIMPSON, on the 6th inst.

The motion was seconded by Dr BURN, and unanimously agreed to.

On the motion of Dr COCHRANE, seconded by Dr JAMES YOUNG, it was resolved that the Society should be represented at the funeral on Friday in its corporate capacity. It was further remitted to the Council to draw up a letter of condolence to LADY SIMPSON, which should be engrossed in the minutes.

MEETING ADJOURNED.

FŒTUS MARKED BY A MALFORMATION OF THE THORAX AND ABDOMEN.

By ALEXANDER MILNE, M.D.

MAY 25, 1870.

DR MILNE exhibited a fœtus between the fifth and sixth months, marked by a malformation of the thorax and abdomen. It was also shrivelled like a mummy, and had probably been dead *in utero* for a couple of months.

CASE IN WHICH THE PRESENTING ARM WAS WRENCHED OFF IN AN UNSUCCESSFUL ATTEMPT TO DELIVER, WITH REMARKS.

By ALEXANDER MILNE, M.D.

MAY 25, 1870.

DR MILNE showed an arm which had been torn off during the endeavour to deliver, and the following are his notes thereanent:—

On the 20th of May last, Dr Groves of Trinity requested me to see a woman who had been in labour for upwards of twenty-four hours, and who had fallen under his care after being abandoned by another practitioner on making several ineffectual, and it must be added, unskilful attempts to deliver. It was a case of twins, and the first child presenting naturally was delivered without trouble. The second was an arm-presentation (dorso-anterior), and the gentleman in attendance, after employing a great deal of strong traction on the arm, wrenched it completely away. It is not known whether the accoucheur previously made any attempt at turning (which of course was the recognised method of procedure in the case), because he remained out of the way after his disastrous failure. It was after this that Dr Groves was called in, and he

arrived about four hours after the arm was severed. Dr G. found the parts so extremely swollen and bruised, and the uterus in such a state of tetanic spasm, and grasping the fœtus so closely and firmly that it was impossible for him to reach a foot and turn. On my arrival about twelve hours after the tearing away of the fœtal arm, I found the state of matters to be as follows:—Patient pale and exhausted; her pulse rapid and feeble. On making a vaginal examination I found the parts preternaturally hot, swollen, lacerated, and besmeared with a dark discharge; the glenoid cavity, with a few ragged shreds of muscle surrounding it, presenting a little below the brim. Dr Groves placed the patient deeply under the influence of chloroform, and I began inserting my left hand with the view of arriving at a foot. This was a very laborious task, the hand having so little room to act, and being so firmly compressed between the uterus and the child. I had to stop my cautious efforts more than once in order to rest my arm, and I thought I should have to desist from the attempt to turn. In about twenty minutes, however, I was able to seize hold of a foot, and thus to accomplish version. The child, I need hardly add, was dead. The woman succumbed on the fifth day from sloughing of vagina and exhaustion consequent on the loss of blood which took place, and the rough and unscientific treatment to which she was subjected before the arrival of either Dr Groves or myself.

REMARKS.—Turning, I need scarcely remark, is in ordinary instances a pretty simple proceeding, but where the liquor amnii has been long drained away, and the parts have become tumefied and dry, and further, perhaps, swollen by the long continuance of the labour, and even lacerated by manipulative interference, the case merges into one of some difficulty. If to these adverse conditions be superadded a tonically spasmodic condition of the uterus, then we have an almost impracticable case to deal with; and it has frequently happened that perforation has had to be performed. Since the introduction of

chloroform, we have been better able to deal with such difficulties, and to ward off the grave operation of craniotomy. It may be safely stated that in this case, but for the anæsthetic, we should have been compelled to perforate. It is true that the result here was just as much to be regretted, but that was doubtless owing to the malpractice of the party who decamped after bungling the case. It will be noticed that the hand employed in turning was the left one. I have used this hand in the majority of cases for the following reasons, which I respectfully commend to the consideration of the Fellows of this Society,—viz., 1. The left hand is usually smaller than the right, and thus more certainly worms its upward way in tightly contracted cases. 2. The canal to be traversed is backwards, upwards, and somewhat forwards in its direction, and the left hand (and fore-arm) adapt themselves to that curve much more easily than the right—*that* is, with the woman lying in the ordinary obstetric position. You cannot adapt the right hand and arm so well without bending the wrist backwards in an unnatural and uneasy degree. 3. The knuckles of the right hand hitch against the pubes, whereas in the case of the left they are received into the cavity of the sacrum. *Lastly.* In employing the left hand, the right is free to render important aid externally either in the way of helping up the head or bearing down the breech. The influence of external manipulation is often very great. Dr Hick's method—the combined external and internal plan—has superior advantage in some cases, such as in placenta prævia and convulsions; and I have reported to this Society several cases showing its value. But where a chain of conditions such as I have just reported existed, the combined plan would be entirely impracticable. There is, however, another method—one of my own—which I have frequently put into practice, and which might be termed the combined internal plan. It is this:—with three or four fingers you grasp a foot, or the knee, and make traction downwards, while simultaneously with the

index-finger and thumb of the same hand you push the presenting part, whether shoulder or head, upwards. A more speedy revolution of the fœtus is thus accomplished.

CASE OF PUERPERAL FEVER.

By James Young, M.D.

May 25, 1870.

My object in bringing this case under the notice of the Society is not to discuss the true pathology of the disease by entering into a lengthened paper on the subject, however interesting such an investigation might be, but merely to narrate the several phases of the case as they occurred, the treatment followed, and the result—almost inevitable in every such case—namely, death.

Mrs A. B. expected her sixth confinement to take place about the middle of April last. I had occasion to see her frequently during the month of March, when she suffered from severe neuralgic pain of one leg, but which was speedily cured by the subcutaneous injection of morphia. I was sent for at 8 P.M. on the 16th of April, but being from home, Dr Stevenson Smith kindly attended till 10.15 P.M., when I saw the patient. Labour seemed to be going on very naturally, the os was well dilated, and the head coming easily down into the cavity of the pelvis. Chloroform was administered, which induced quiet sleep, but was followed by vomiting. The pains were natural, and the child (a boy) was born at half-past eleven o'clock. The placenta was quite adherent, and had to be removed with the hand. I remained with my patient till 1 A.M., when I left her quite well and comfortable. On the 17th, at 1 P.M., she was very well and cheerful, and continued so, sleeping well till the forenoon of the 18th, when, from causes to be mentioned afterwards, Mrs A. B. was seized at ten or eleven o'clock with a severe rigor, which was immediately followed by local pain and heat of skin.

When I saw her at half-past eleven she was complaining of considerable pain over the region of the uterus, with the pulse at 100. I immediately ordered ʒss. of castor-oil with fomentations. At 7 P.M. the bowels had acted, but she was no better. I advised the ordinary doses of calomel and opium, and turpentine stupes to be applied. On the 19th I saw her four times; the first visit was at 10 A.M. Her pulse was then 104, pain somewhat moderated, considerable thirst, no more rigors. At 10 P.M. the symptoms were much the same. Calomel and opium continued, with fomentations. Milk and arrowroot were given her frequently, and ice at intervals. Early on Wednesday, 20th, I found the pulse up to 110, and a tympanitic condition of the abdomen commencing. She complained very much of a flatulent and painful distention of the bowels. There was no diarrhœa, and no vomiting up to this time; on the contrary, she took beef-tea and milk with some relish. From a rising pulse, tenderness over the abdomen, and tympanitic belly, I feared an unfavourable issue, and asked Dr Keiller to see her with me in consultation. We saw the patient at half-past 11 A.M. We prescribed opium without calomel in grain doses every two or three hours, with constant hot stupes all over the abdomen till 10 P.M., when we again met. Mrs A. B. was not much worse at this time; but she had had no sleep, and had once vomited some of the brandy which had been ordered in doses of ʒss. every three hours. On Thursday morning (21st April) I saw her at five o'clock, and found the indications of depression increasing, and ordered rather more brandy. The stomach was becoming irritable from the enormous distention of the belly. My patient began to vomit about once or twice every twelve hours. The alvine dejections, as well as the matter vomited, were becoming blackish. We recommended the continuance of the ice and brandy frequently, with fifteen drops of morphia in water every four hours. On Friday morning there was no improvement either in strength or in the pulse, while the abdomen continued tender and increas-

ingly tympanitic. At this stage, I ordered a tissue blister over the surface of the bowels, but without any good result. The pulse on Friday at mid-day rose to 124, and was rather weaker, with occasional vomiting; the patient continued perfectly sensible, answered questions, and never complained of headache, nor of flying pains in the muscles till this morning. Symptoms of a fatal termination supervened on Friday night, when the pulse rose to 130-140. At 4 o'clock A.M. on the 23d, Saturday, she had a severe fit of vomiting of dark bilious matter, with indications of sinking. At 7 A.M. I ordered a tablespoonful of brandy every half-hour. At 10 A.M. the pulse was 160, and extremely feeble. At half-past eleven forenoon, she placidly and gently died, having verily "fallen asleep."

The lochial discharge was generally fœtid and scanty. There was no secretion of milk. After her fourth confinement, Mrs A. B. was attacked with phlegmasia dolens, from which she made a slow but good recovery. In order to illustrate the effect of mental distress in inducing fatal disease, I must here mention some facts in connection with this case, alluded to in the commencement of this paper. Suffice it to say that the patient suffered great grief and distress of mind for months previous to her last confinement. I cannot here specify very particularly the exact nature of her grief, lest I tread, as I fear I would do, on the domain of domestic affairs. The patient was kept in constant anxiety and fear. Moral delinquency on the part of a near relative was another cause of her distress of mind. Let me only further add, that, on the morning of the second day after her confinement, when she had awoke from a long and tranquil sleep, she had put into her hand ten or twelve letters (at her own previous request), all of which she read. Almost immediately thereafter, having apparently suffered from much excitement, Mrs A. B. complained of a severe rigor, followed by the symptoms already mentioned. Whatever we may have to say concerning the pathological cause of

puerperal fever, there can be little doubt that, in this case, it commenced in acute metritis, the inflammation extending to the peritoneum.

Dr Pattison said that he had seen two cases of puerperal fever, which he believed had been caused by mental distress. In these cases the lochial discharge was very offensive, and the secretion of milk was arrested.

Dr Cairns had met with only one case, in a patient who had a phthisical tendency. She did well for some days; but after getting up and going into the kitchen one day she was seized with a rigor, and then mania supervened. She was very restless and much excited, and all the symptoms of puerperal fever showed themselves in two days. She succumbed in eight days. He thought, in this disease, that calomel and opium were doubtful remedies, seeing that the brain was usually affected. He had relied chiefly on champagne and beef-tea. He tried chloral, but did not see much benefit from it.

Dr Cochrane looked on calomel and opium as our sheet-anchor in puerperal fever; but fomentation, turpentine, enemata, and ice, were of much benefit.

Dr Coghill thought that Dr Young's case was a typical one of a large class. There is a great variety in the pathological nature of the cases. Dr Young's was one of that large class dependent upon local uterine irritation; others, however, were the result undoubtedly of a specific blood-poison. In one case he had seen lately, there was a distinct history of smallpox contagion, and the mother died four days after her confinement. The child was marked with smallpox. He thought that Dr Young did right in giving in such a case small and repeated doses of calomel and opium; and he himself had found benefit from the administration of frequent enemata of opium in barley-water, administered as warm as could be comfortably borne. In most cases of blood-poisoning, stimulants were absolutely necessary; and turpentine applied externally, and followed by a compress steeped in Battley's solution, gave marked relief.

Dr Murray said he had met with three cases of puerperal fever, and in all of them the symptoms were rather deceptive. There was no pain, but the patients were

restless, the belly distended, the eye glassy, and het pulse peculiar. He said he was inclined to think that the inflammation of the uterus had nothing to do with the fever.

Dr Burn was of opinion that, when there is much local pain, leeching did good. In many cases, however, there is no pain, unless deep pressure be made with the fingers over the region of the uterus. In the cases which had come under his notice the head was never much affected. He remembered seeing one patient die five minutes after putting out her tongue. In local inflammation calomel and opium, he thought, were the best remedies; and he usually noticed that if a woman's mouth became affected by the mercury she was sure to recover. He believed that in many cases the mischief begins before labour comes on.

Mr G. Stevenson Smith said that, when in Paris in 1859-60, he had seen a dreadful epidemic of puerperal fever in the wards of M. Briquét, in La Charité. The treatment consisted chiefly in the administration of very large doses of quinine, but the patients all died.

ON THE EFFECTS OF CHLORAL.

By Thomas Cairns, M.D.

May 25, 1870.

My principal reason for bringing this subject before the Society is to elicit the opinions and experience of the Members upon it, and thereby be able, in some measure, to determine whether or not I should continue or discontinue the use of the drug under consideration. "Omne 'novum' pro magnifico,"—Everything new commands the admiration of the multitude. And chloral being a new drug must, according to that aphorism, necessarily prove a panacea for almost all diseases which affect the human frame. And if, in these circumstances, it is not administered by a practitioner for this, that, and the other disease, he must, as a matter of course, incur the odium either of being far behind in his profession or of culpable neglect.

It is for these reasons that I trouble the Society with

this paper, which I shall endeavour to make as short as possible.

The method which I propose to adopt is simply to state in their order the different cases in which I exhibited the drug in question, in what doses, and with what results. And the first case I have to mention is that of—

1. *Spasmodic Croup.*—The little patient was about two years of age, and suffering from dentition, on which the other symptoms evidently depended. Chloral in 3-grain doses thrice daily was given for a couple of days. There ensued no abatement of the symptoms; and as the physiological effects were such as to attract the special notice of and to alarm the mother, I judged it prudent to discontinue the drug. The effects alluded to were great restlessness, incessant tossing of the arms, staring of the eyes, increased action of the heart, and hurried respiration, and, as the mother expressed herself, "Baby was not at all like herself."

2. *Chorea.*—This was the case of a girl of the age of 14. All the usual symptoms were well marked. To this patient the drug was exhibited for a fortnight in 10-grain doses thrice daily, without producing any apparent effect whatever, either therapeutical or physiological.

3. *Facial Neuralgia.*—The patient was a house-carpenter, and of 30 years of age. He had been ill for about eight days before I was called to see him. He had never had the same complaint before. The attacks had recurred regularly every afternoon at about four o'clock, and had continued up to about ten at night. Was ordered a smart purge immediately. On the following day I made it a point to see him at the hour at which the attack usually began, and found him with his head pressed between his two hands, and unable to speak from the pain he was suffering. Was ordered the following mixture:—
℞ Chloralis hydrat., ʒ ss.; Liq. morph. hydrochlor., ʒij.; Decoct. guaiaci ad ʒ vi. F. M. Sig., Cap. coch. mag. quaque horâ quartâ.

Of this mixture it will be observed, that each dose con-

sisted of 20 grains of chloral, 10 minims of the solution of the muriate of morphia, and nearly half an ounce of the decoction of guaiacum. Next day the attack did not come on till six o'clock,—two hours beyond the usual time,—and lasted only three hours instead of four. To be brief—he was perfectly well in three days, and on the fourth was able to resume his usual duties. After the strictest inquiry, I could not get this patient to acknowledge that he had felt anything peculiar while under the influence of the drug in question.

4. *Sciatica.*—In this case the patient had a sound sleep by a nightly dose of half a drachm of chloral; but as soon as she awoke the pain was felt as acutely as before. This continued for three weeks. The drug was given up entirely. Since then she has gradually improved under the use of quinine and aconite combined. In this case there have been manifested no physiological effects, although the dose consisted of half a drachm each night.

5. *Delirium Tremens.*—I have tried chloral in two different cases of this disease, and with very different results. In one case under its use the symptoms were aggravated, at least apparently so, to a great degree, and also prolonged very much beyond the time they usually continue. In the other case, the symptoms abated shortly after the administration of the first dose, which in each case was half a drachm three times daily. In the unfavourable case the symptoms continued for three weeks,—the chloral, however, having been discontinued by the end of the second; whereas, in the other case, they had entirely disappeared by the end of the second day. In neither of these cases were any physiological effects at all observable.

6. *Hysteria.*— This is the case of a young lady who has been married for three years; has always been "regular" up to the present time. Cries and laughs alternately at times, without being able to account for such conduct. Ordered a drachm of chloral in two ounces of a solution of starch, to be thrown up the rectum at bedtime. I called on the following forenoon, and found her in bed, and was

thus accosted by her,—"Oh! doctor, you have killed me. You have made me a great deal worse than I was. Shortly after I got the injection I felt as if I were between heaven and earth, and as if no power were in my whole body. I have not slept a wink the whole night, and at this moment I feel quite powerless. And I may also tell you this, that I have seen so many ugly creatures flitting before my eyes that I am positively afraid to look up." I stopped the chloral at once, and I have not ventured to try it again upon her, even per rectum.

7. *Irritability of the Bladder.*—This was the case of a gentleman who resides in the country. The urine having been reported to be high coloured, and to deposit a brick-red sediment on cooling, he was treated for a very considerable time for the uric acid diathesis, but without any favourable result. Experiencing much discomfort, especially during the night, I advised him to pay a running visit to town, which he did, when, upon examination, I found that the affection of the bladder depended upon irritation of the rectum, due to internal hæmorrhoids. The patient, having neither time nor inclination to undergo the radical, was put upon the usual palliative, treatment, in which he persevered for several weeks, but without experiencing almost any benefit therefrom. He was then ordered half a drachm of chloral each night at bedtime, which, without having produced any disagreeable effects, so far as he has been able to observe, has invariably procured him a comfortable night's sleep and rest. He had taken it for a week when I last heard from him, which was a fortnight ago.

8. *Cancer of the Uterus.*—Chloral was given in this case only as a last resource, after numerous constitutional and local sedatives had lost their effects. It was exhibited in scruple doses four times a day. By the time the patient had taken the fourth dose, the symptoms became so alarming as to render necessary the entire discontinuance of the drug. The symptoms alluded to were, apparent deprivation of consciousness, violent heaving of the chest,

lividity of the face, constantly rising up in bed, and tossing of the arms and legs; head squeezed forcibly between the hands, as if the seat of great pain; incoherency of speech.

9. *Coccydynia.*—Chloral was at once administered in this case, and in drachm doses every second night, but in the form of enemata. I saw the patient, who was a young married lady of a very nervous temperament, in the forenoon of the day following the exhibition of the first dose, and the account she gave me was, that in the course of fifteen minutes after she got the injection, she felt "as if every particle of power had gone out of her body, and that if she had not been in bed she must have fallen on the floor from utter prostration." After having presevered in the use of the drug for eight days, and with the same results as above described, I judged it expedient to discontinue it.

10. *Uræmic Convulsions.*—The patient is a married lady, 38 years of age, and has had three children, all of whom are alive and in good health. She has always been "very regular." She is still so. Her youngest child is three years of age. About four months ago she complained of slight swelling of the feet, but nothing more. I thereupon immediately secured a little of her urine, which, on the application of heat and nitric acid, became converted into an almost white solid mass. I put her immediately upon half a drachm of Tinct. ferri hydrochlor. thrice daily, under the use of which she seemed to improve, judging from the urine and the state of her general health; when, by-and-by, I was sent for in great haste to "see her, because she had taken fits." I was present shortly after I was sent for. When I arrived I found her labouring under a severe and protracted fit indeed. I remained by her bedside all night, and tried the usual remedies, but without any apparent benefit. The convulsions recurred sometimes at fifteen, and sometimes at thirty minutes intervals. On the following day I called in Dr Keiller for consultation, who ordered half an ounce of chloral to be given in drachm

doses per rectum every half-hour. The drug was administered by my own hands as directed, and with beef-tea as a vehicle. Twenty minutes after the first dose was given she became very restless. She rose up in bed every few minutes, threw her arms about her, pressed her head between her hands, and exclaimed, "Oh, my head!" tossed her legs incessantly, rendering it necessary to hold them together by an attendant. Her face became of a purple hue. The action of the heart increased to such a degree as to alarm three or four people who were present. The fits continued. I persevered with the drug until three drachms had been given, when, on finding the patient all but pulseless, I felt obliged to stop it. I then left, expecting to find her dead in a few hours. Six hours after, a message was sent me to the effect that Mrs S. had had a pleasant sleep, and that she was very anxious to see the doctor. In short, she recovered; but three weeks after, the convulsions recurred, which lasted only a day and a half instead of three days, as on the first occasion. On the second occasion no chloral was given. The patient appeared to make rapid improvement under large doses of acetate of potash and bromide of potassium—judging from the character of the urine, which, about five days before death, contained almost no albumen whatever—when there ensued symptoms of another attack of convulsions, which, in two days after, actually occurred, and which resulted in the death of the patient two days thereafter, which was on Thursday last. This case, which presented several points of peculiar interest, I shall probably bring before the Society in a separate form on some future occasion.

11. *Autopīra.*—This term simply means an experiment upon one's self, and I have used it only for the sake of convenience, because it fully expresses all I have to say under this head. The experimenter is a medical man in this city. He was induced to try it upon himself in comparatively large doses, with the view of ascertaining to what extent it could be pushed up with safety in the case of patients.

One night a drachm was taken, the second night two drachms, and the third night three drachms. The symptoms produced were the same in each case, but differing, of course, in degree. These symptoms, after the exhibition of the largest dose, were, and in the course of ten minutes, great confusion of mind—a feeling as if the subject were now in one county and now in another, and with an anxious desire to get home; a consciousness of everything going on around him, with an inability to resist giving utterance to what he knew to be profound nonsense; most extreme restlessness; action of the heart so violent that its pulsations could be heard at a distance; respiration so hurried as to "resemble the panting of a dog;" face quite purple; eyes bloodshot, and as if starting out of their sockets; perspiration in large beads on the forehead. The drug was taken at eleven at night, and the symptoms above enumerated continued without intermission till six in the morning, when, apparently from sheer exhaustion, the subject became quiet and fell asleep, and slept for two hours, when he awoke with a most violent headache. For three weeks thereafter there was felt a painful sense of drowsiness; the eyes, also, were much inflamed, and unable to bear the light without pain; and the temper—never very good—has been much more irritable than usual ever since.

Such are the chief cases which have come under my observation in connection with chloral. I now conclude with a few remarks.

1*st.* Judging from the marked action of the drug on the heart and lungs, I would infer that it would be somewhat hazardous to administer it to a patient labouring under disease of either of these organs.

2*d.* It differs greatly in its action in different individuals. In illustration of this let me allude to the very different effects which it produced in the two cases of delirium tremens which I have mentioned; and I could adduce many other cases of a similar nature.

3d. It produces different effects in the same individual at different times. Of this I shall give only one example. The practitioner who experimented with it upon himself on three successive nights, and invariably with bad results, tried it once more about a week ago, on which occasion it induced a long, unbroken, and refreshing sleep.

4th. It appears to be most useful in the different forms of neuralgia. In one of the two cases which I have reported, it yielded, or at least appeared to yield, very speedy and very favourable results. In the other, the effects were not so satisfactory, but in neither case were there produced any bad effects.

Lastly. As a pure and simple anodyne and hypnotic, it is, in my opinion, very much inferior either to cannabis Indica or opium, or either of its alkaloids. In one of the cases of delirium tremens above mentioned, after a fortnight's use of chloral, with no good, and several bad effects, a single drachm of cann. Ind. induced six hours' unbroken sleep. Under less doses of the same drug every night, combined with potass. brom., the patient gradually improved, and was able to resume his duties a fortnight thereafter. Moreover, in none of the cases, except two, which I have brought before your notice this evening, has the drug in question acted in the least degree as a hypnotic, and in one of these cases the subject had been for two successive nights out of bed, attending to professional duties, thereby rendering it at least probable that the sound sleep which he enjoyed was not due to, but in spite of, the effects of chloral.

Dr Gordon had prescribed it in a great many cases, in doses of 10 to 20 grs. repeated every two hours. The patient frequently complained of pain in the eyes.

Dr Keiller said that he had used it sufficiently often to know something of its effect, which he found to be variable. As a hypnotic, he had found it very serviceable. From the several cases of labour in which he had seen it employed, he would not be inclined to regard it as

a valuable agent in obstetric practice; and his experience of chloral led him to conclude that it would not in any degree supersede the use of chloroform in midwifery practice.

Dr Cochrane said that he had used chloral with very good results. In a case of rheumatism, he had found it very beneficial, prescribed in the following manner:—℞ Chloralis, ʒi.; Syrupi tolutani ʒi. M. Half of the above was taken, and it was successful in inducing a sleep of eight hours without intermission. On awaking, the patient felt much refreshed. On the following night he took the rest of the draught, and enjoyed a good sleep, but complained of giddiness, and a peculiar sensation over the head. He had tried it in a case of concussion of the brain, as well as in a case of convulsions. In the first case, sleep was superinduced; in the other, the convulsion ceased. At the same time, he had seen other cases in which harm rather than good was done.

Dr Coghill said that he had been disapppointed as regards the range of its utility. He had made use of it with remarkable success in all cases where opium disagreed. He had found it to act very well in all purely spasmodic complaints. In two cases of spasmodic asthma, he had found it to be very beneficial. It did not do to be repeated, but you should allow the first dose to be eliminated, in say a period of two or three nights. He considered that in a case of continued fever, where opium was contra-indicated, it would prove beneficial. In some cases, he had found an alarming lowering of the pulse, which sometimes went down to 60 per minute. In puerperal mania, he considered it would be found most useful, if not specific. In two cases of rheumatism, he had employed it with good effect. He had noticed that when combined with bromide of potassium, its hypnotic effects were increased. In one case, a single dose, when thus combined, had sufficed to keep the patient in a hypnotic state more or less for nearly a week—so much so, that he became alarmed. It seemed to increase the flow of urine materially. He was the first to point out its effects as an irritant upon the mucous membrane of the eyes. In some cases, a sort of muscular neuralgia had followed its use. He had given it in the convulsive diseases of children at a very early age. For example, in a case of paralysis nutans in a child a fortnight old, he prescribed

two grains, with good results. In cases where the hypnotic effect was not produced, it gave rise to a low form of delirium. In delirium tremens, it was given with excellent results. When given as a suppository, or as a pessary, it causes violent irritation, not inflammatory, however, with profuse discharge from the bowels, but its action was modified by the combination with oxide of zinc. When employed endermically, it frequently gave rise to considerable irritation, and even sloughing of the cellular tissue. He had found that it almost invariably produced its hypnotic effects very rapidly, not above ten minutes passing before sleep was induced. It did not constipate at all; and he had found that in nearly all cases the appetite was greatly improved.

Dr Ritchie stated he had used chloral several times both in Children's Hospital and private practice. He had not, however, tried it in any case of labour. In a case of delirium tremens in a male adult, he had given it with the following result:—The patient was suffering from a second attack, there having been an interval of about six weeks between it and the preceding. The first attack was treated with bromide of potassium chiefly. On seeing the patient in the second attack on 15th April last, ten grains of chloral were ordered every four hours. The drachm ordered was, however, taken in the course of six hours, but produced no result. The next day (16th) the patient being no better, one grain of opium was given every four hours; six grains in all were taken, but only caused sleep for half-an-hour. On the 17th, he seemed better, being more composed, and having less tremor. Not caring to push the opium further, bromide of potassium, which had been used successfully in the previous attack, was ordered in 20-grain doses, every two hours. At night, half-an-ounce had been taken, but no sleep had resulted; the eyes were more injected, and there were greater restlessness, and more delusion; 1 grain of opium was then combined with the potass. On the 18th, he was worse, had passed a very bad night; no sleep, and had taken 2 drachms more of potass and 6 grains of opium. The potass was stopped, and 6 grains more of opium taken; but in the afternoon, as they had caused no sleep, 20 drops Liq. opii sedativ., every second hour, were given. On 19th, had taken 100 drops in all; passed a very bad night, no sleep; was

very restless; hallucinations of seeing, hearing, and feeling, during night. At visit he was rational; pulse 100, becoming weaker; insisting on getting up; half a drachm of chloral put him to sleep in ten minutes; he slept for half-an-hour. The dose was then repeated, and he slept more or less during day, and after the third dose during night. After this, he steadily improved, and required no further hypnotics of any kind; the case was complicated with hepatic congestion.

In another case of chronic hepatization of right lung, the patient had frequent spasmodic cough, preventing sleep; scanty expectoration, great prostration of strength, and night sweats. Bowels were regular, but there was great irritability of stomach, so that remedies required to be given with great caution. He suffered much from want of sleep; opium disagreed, and chlorodyne he could not take. 20 grains of chloral were given, he slept quietly, and had no sickness. The second time the sleep was not so sound; and after the third dose, 20 grains did not produce much result. Six doses of 30 grains each were ordered, and directed to be increased, if necessary; the first had the effect of producing complete prostration of consciousness. He coughed and sat up in bed during night, and had no recollection next day of anything that had passed during night. There was no sickness, however, but the effect produced on the patient was such that he compared himself to having been dead, and therefore objected to take the chloral again. He was, however, persuaded to take 30 grains. He did so on two occasions with the same result, and would not again repeat it, as he was so thoroughly helpless when under its influence. The results in this case were satisfactory so far as the production of sleep was concerned. It had no effect in moderating the sweats, but caused no unfavourable symptoms or consequences. Dr R. had also used it in several cases of the delirium and pervigilium of typhus. In one hospital case, the child had lung complication, the conjunctivæ were considerably injected, and the night delirium and restlessness great. 10 grains of chloral, repeated for three nights, had a very good effect. In an adult, with typhus, with lung complication and mild delirium, its effects were very satisfactory. He had also given it in a case of typhoid fever, with eruption, in which the prevailing symptoms had been retching and vomiting, with pain on left side of abdomen, and sleep-

lessness, with occasional but very slight delirium. In this case, in 20-grain doses, it did not cause sleep, yet it occasioned no sickness. He thought the hydrate of chloral was a very valuable and useful drug, and that its want of success in certain instances was not due to the drug itself, but to its being administered in unsuitable cases. He considered that much had still to be learned regarding the cases in which it was suitable, and the dose in which it should be given. It appeared to him that it was not suitable in every case, and a further experience of the remedy was required before it could be exhibited with certainty as to its effect; but so far as his still limited experience of it had gone, he had certainly found that no injurious results had been produced.

After Dr Ritchie's remarks, it was agreed to adjourn further discussion of the subject till next meeting.

CASE OF CEPHALOTRIPSY.

EXHIBITED BY ALEXANDER KEILLER, M.D.

JUNE 8, 1870.

DR KEILLER exhibited a very interesting case of cephalotripsy to the Society. A young girl in the Maternity Hospital had been in labour for several days. She had a very contracted pelvis. As the head was still above the brim, it was necessary to do something. Finding it impossible to deliver her either by the long forceps or by turning, he considered it a good case for cephalotripsy, which was successfully had recourse to by means of the cephalotribe he had used in previous cases. On a former occasion, he had stated that in the recent cases in which he had employed the cephalotribe, he had found the Edinburgh instrument amply sufficient. He thought the Members of the Society would see, by looking at the instrument and child in this case, that he had succeeded perfectly in diminishing the size of the head opposite the contracted part of the brim. Dr Inglis had

said that he had failed in crushing the malar bones; but this was not absolutely necessary, because the face was not usually opposite the contracted part, and in the present case, where the brim was contracted, there was plenty of room at the sides of the pelvis. The great point to aim at was to diminish the size of the head, so as to bring it through the narrow brim. He, Dr K., exhibited the child, with still applied instrument, as it had been extracted. The flexion of the head, and application of the blades beyond the base down to the neck, and the crushing power, were perfect. The side view was the best. In these cases, perforation was first adopted. After the head was extracted, the pelvis was so small that the body of the child required to be delivered by instruments. A hook was accordingly applied in the axilla of the child, where it had been pierced. So small was the pelvis, that the placenta had also to be removed through the brim. He considered that this was a good example of the powers of the so-called Edinburgh instrument. The mother had an inflammatory attack of the cervix, which was slightly lacerated. The pulse was quick, but she was going on favourably. She was delivered on Monday night.

Dr Bell said that, along with the cephalotribe, the child weighed 12 lbs.

Dr Cairns thought that if the blades were made a little broader, and the corners a little longer, you would have a greater leverage power.

Dr Keiller said that no doubt this instrument could be easily modified and improved, but, as regards essentials, it was the kind of instrument necessary for the purpose. Sir James Simpson tried to improve the blades by making the one a little shorter than the other.

ON THE INFLUENCE EXERTED BY CHLORAL ON THE PAIN OF PARTURITION.

By E. LAMBERT, ESQ., PARIS.

JUNE 8, 1870.

THE subject to which I have the honour of drawing the attention of your Society will possess a peculiar interest in your eyes from the fact that it is directly connected with the name of one so justly memorable as Sir James Simpson. It will command this interest at least, that it purports to bring before you and before the profession the facts known to myself in connection with the latter researches of Sir James Simpson in the direction of "obstetric anæsthesia," and I cannot but hope that it may be in the power of others to supplement these by many other observations.

I deeply regret that the few facts here narrated could not be brought collectively under the cognizance of his master-mind, and that I was not permitted to take counsel of his mature experience as thoughts forced themselves upon me during the prosecution of these researches.

When chloral was brought before the profession, Sir James was foremost in prosecuting inquiry into its therapeutic value. I do not know whether he had applied it in private obstetric practice, but his paper on chloral, of 1st January, in the *Medical Times*, makes reference to the subject. He states that he has seen the parturient womb contract regularly whilst the patient was unconscious. During the last fortnight of the year 1869, he had attended, with my colleague Dr Walker, a case in charge of Dr Somerville, but I have no notes of the results. On the 31st December I requested his attendance; the case is related under No. I.; there was absolute intolerance of the medicine. The next case occurred 2d January 1870, and the important results given in No. II. were observed.

The last case attended by Sir James Simpson, in connection with the Maternity practice, dates 4th January, No. III. Chloral, it was apparent, could not claim to supplant chloroform, since it abolished consciousness to a less extent, placing the patient as it were midway between conscious and unconsciousness, and rendering her incapable of that control which is essential during the close of the second stage; but this admission only placed in a stronger light the admirable properties of the agent when applied to the relief of pain during the first stage of labour, at a period when it is generally conceded that chloroform is hurtful.

As *the* hypnotic of this first stage, chloral stands as yet unrivalled; we have only to remember that opium, our only sure refuge, must be administered with the knowledge that we are conspiring, though for a higher end, against the course of labour.

It is with an earnest wish to further the progress of our art that I invite the attention of your Society to the bare statement of the whole facts which have come under my observation. None more than yourselves will feel the interest that attaches to researches in this particular direction—none, I am sure, will be more anxious to employ the keen edge of criticism to the profit of experimental science.

CASE I.—Bridget M'T., aged 25, in her second pregnancy. was admitted to the Maternity Hospital at 8 A.M., 31st December 1869. The os was fully dilated, the membranes entire, the head in first position, resting on the brim of the pelvis. The pains, carefully noted, were recurring about every five minutes from 8.50 A.M. to 10. Sir James Simpson, accompanied by Dr Jenkins, U.S., and Dr Coghill, attended at 10.15, and chloral 3i. was then administered. It was vomited six minutes after; and, at 10.28, 3ij. were given in an enema; this was voided five minutes after. At 10.40 the patient had slight rigors, and from 10.45 to 11 there was marked somnolence in the intervals of pain. She then said she felt sleepy,

but could not sleep, and this sensation passed away shortly. At 11.18 ʒi. was given and vomited immediately. This was followed by rigors and somnolence, as above, for the space of fifteen minutes. At 11.45 ʒij. were administered in an enema, but voided during the next pain. At 12.15 the membranes were ruptured artificially. From 12 to 1, the bowels were moved twice. The patient had constant eructations, and vomited twice. The labour terminated at 3.45; the pains remained throughout efficient.

CASE II.—Agnes P., aged 22. Primipara. Admitted to the Maternity Hospital at 6 P.M., 2d January 1870. At 7.30 the os was the size of a half-crown; the head, first position, was entering the pelvis, the membranes entire. The pains were strong, at very regular intervals of five minutes. In the intervals of pain has constant slight rigors, which become much stronger during the pain. At 9.25 the membranes ruptured spontaneously, and at 9.30 the head was on the perinæum.

Sir James Simpson attended at 9.50; chloral ʒi. was then administered. 9.55.—Says she feels the pains less. At 10, ʒi. again given; the rigors ceased, and the patient seemed to sleep in the interval of a pain. At 10.4, she is remarkably calm. At 10.9, there was a slight rigor, then a severe pain, and a sharp cry; and at 10.10 the head was born. Answered questions; said the pains were much less severe since she took the medicine, and that she slept in the intervals. 10.30.—Sleeping very quietly, answered a question, and fell off again immediately. Removed placenta from vagina without any indications of consciousness on the part of the patient. 3.30.—Woke and said, "Where am I?" Hearing her child cry, said, "Whose child is that?" Did not remember birth of child; took food, and slept soundly till morning. Questioned next morning, she affirmed that she only remembered looking up and seeing three doctors; remembered taking the medicine and nothing more; knew nothing of the birth of the child.

Dr Gordon accompanied Sir James Simpson. She made an excellent recovery. Her child had spina bifida. The case was reported to the Society by Dr C. Bell. Chloral was administered to the infant, whose horrible cries were quieted by it; the spasms were also stopped by its influence.

CASE III.—Margaret O., aged 26. Primipara. Admitted to the Maternity Hospital at 5 P.M., 4th January 1870. Had felt pains since 7 P.M., 3d January. At eight A.M., 4th January, saw a doctor, who told her she was in labour. Took train from Stirling, and was directed to Maternity, Edinburgh.

4th Jan., 5 P.M.—Os size of a shilling, rigid. Membranes entire. Head, first position, entering pelvic cavity. Pains strong.

5th, 12 noon.—No progress. Pains apparently very severe. Patient much excited during their recurrence, and very unruly. Moans constantly during intervals. Has refused food for twenty-four hours. Pains strong, but intermissions irregular, and pain never absent. Sir James Simpson attended at 12.45. Chloral ʒi. was given. 1.7.—Marked somnolence during intervals of pain. 1.18—Quiet sleep during intervals. She always gets on her knees during the pains. These appear just as severe, but the intervals are at least two minutes longer, and very regular. She always re-assumes the supine posture before the pain is over, and falls asleep again immediately. 1.48.—Says the pains are very sharp, and complains that medicine is doing no good. Has much difficulty in getting on her knees. Her movements are unsteady. Eyes fixed, haggard; equilibrium uncertain. Ordered not to leave the supine posture. 2.35.—Pains very severe. Still regular. Moans during intervals. 2.48.—Chloral ʒi. 3.10.—Perfect repose during intervals of pain. 4.30.—Again becoming agitated and anxious. 5.30.—Os dilated. 7.—Dr Walker ruptured membranes. 7 30.—Chloral ʒi. From 7.30 to 10, when the head was born, this last dose remained efficient. There was always

quiet sleep during intervals, and the pains were not very sharp. At the exit of the head she showed no sign of greater pain. When all was over she said that the medicine had given her great relief, and pointed especially to the sleep she enjoyed in the intervals. Made same statement next morning.

Observation.—The marked effect in this case was to bring about perfect regularity in the recurrence of pains. These occurred at intervals sensibly longer, and I think it may be fairly stated that the patient was not fully conscious for more than two-thirds of the duration of the pain.

CASE IV.—Mrs C., aged 40; tenth pregnancy; 2 Campbell's Close, Canongate. *8th January* 1870.—Attended case of Dr Somerville's. Head at brim, first position. Os fully dilated. Pains rare and weak. Dr Somerville reported that she was making no progress for some time back. 1.20 A.M.—Gave Ext. ergot. liq. 3i. 1.32.—Head has entered cavity. 1.35.—Gave chloral 3i. 1.40.—Full, deep respiration, and perfect quietude. 1.45.—Pain apparently less felt. 1.50.—Strong pain. Head on perinæum. 1.55.—Gave chloral 3i. 2.—Sleeping. 2.8.—It is not very evident that she is under the influence of the medicine. Pains follow close upon each other. 2.15.—Head born, much apparent pain.

Observations.—There was a drunken husband, four children, and constant noise and talking. Impossible to obtain quiet, even long after child was born.

8th Jan., 1 P.M.—Stated that she was conscious throughout. Felt very dizzy after second dose, and wished for sleep. Slept well the remainder of the night, and has done well.

CASE V.—Margaret M'D., aged 18; primipara; 7 Gullan's Close. In labour since 12 P.M., 12th January 1870. Vomiting a good deal. 4 A.M.—Found os dilated; membranes ruptured; head below brim. Pains regular—every three minutes. Rectum loaded. Enema.—6.30.—Pains neither regular nor of any duration. Patient very

restless; gave chloral 3i. 6.40.—Strong pain after quiet sleep. Complains little during pains, and is very quiet during intervals. 7.5.—Head on perinæum. Gave chloral 3i. 7.10.—Deep sleep. 7.40.—Muttering. Is much agitated. Cannot be kept to any position. Slight delirium. Mother got uneasy; said there was not fair play. Went out, and returned with a midwife. 8 A.M.—By this time the patient was quite quiet. Got rid of the midwife. 8.20.—Child born. Very little exhibition of pain. Cord over shoulder. Child born without much life. Was liberally *spanked*. Well recovered in three minutes. 8 P.M. —Had slept till a little ago. Remembered nothing, but "skelpin' the backside" (*sic*). Has done well.

CASE VI.—Mrs F., aged 34; eighth pregnancy; 6 Foulis Close. 13*th January* 1870.—Sent a nurse to the case, and attended it at 10 P.M. Os size of a shilling. Pains very poor; not regular. 11 P.M.—Hardly any progress. Patient has been living on oat-meal, and drinking a great deal of water. Has constant eructations. Ergot 3i. produced instantly an emetic effect. A quantity of very sour-smelling fluid vomited, producing great relief. Pains not improved. 11.25.—Ergot 3i. retained. 11.35.—Vomited a little. 11.50.—Good dilatation. Pushed up anterior lip. 12.15 A.M.—Birth of a female child. Another in utero. Membranes entire. Head presenting. 12.25.—I now gave chloral 3i., trusting to it to get over the second birth unknown to the patient. 12.45.—No progress. Head still at brim. Ruptured membranes, and gave chloral 3i. 12.52.—Vomited, and continued to vomit frequently. 1.15.—Head at outlet; forehead presenting. The patient was not delivered until 5 A.M. of a female child, dead. I applied forceps in presence of Dr C. Bell, Dr Walker giving chloroform. The patient was quieted for some time by chloral. She was sub-delirious for about half an hour. She slept a good deal.

CASE VII.—Mrs B., aged 19; primipara; 25 College Wynd. Menstruated 5th July 1869. Married 9th July 1869. Fell, going up stairs, 20th January 1870. Woke

with pains, 22d January, 8 A.M. At 4 P.M., I found both feet presenting *sacro-posterior*. 4.30.—Right foot near vulva. 4.40.—Gave conjointly ergot ʒi., chloral ʒi. 5.10. —Child born, with exomphalos size of half a large hen's egg; cord 1½ inches broad. Child 6½ months. Appeared dead; after twenty minutes of continued efforts it was resuscitated; it lived thirty hours.

CASE VIII.—Anne A., aged 27; second pregnancy; 14 Cowan's Close. At her first labour five years ago, twins, seventh month; had chloroform in the Infirmary, where she was under treatment.

7th February 1870.—Case of Mr M'Swiney. I attended at 7 P.M. Breech *sacro-anterior*. She was a terribly unruly patient, and howled at the top of her voice during her pains, although they were not prolonged; she never ceased to ask for chloroform. At 8 P.M., the breech being well in the cavity, I gave conjointly ergot and chloral āā ʒi. At 8.4, a prolonged pain, with no exhibition of suffering. At 8.7, making a sort of humming noise. For the next ten minutes, she was in a very excited state, rolling her head from side to side on the pillow, throwing her arms to and fro in cadence, accompanying the motion with an extraordinary gutteral noise; inspiration and expiration both sonorous, the first very short, the second much prolonged; now and again a wild cry at the top of her voice during the pains. Within half an hour from the administration, she was herself, and continued her screams till the birth of the child at 9 P.M. Except whilst she was delirious, she continued to ask for chloroform. The child was at full time; the fœtal heart was getting weak when I gave ergot. The labour was not hastened by it, and I could not find any trace of life when the child was born.

CASE IX.—Janet B.; primipara; Royal Maternity Hospital. Patient had been suffering for twelve hours, when I was called to see her at 4 A.M., 15th February 1870. Os just admitting extremity of finger; thin, soft; membranes entire; pains regular and strong. 9 A.M.—Pains are still strong, but recur at rather longer intervals

(¼ hour). No change in condition of os. 10 A.M.—As above, patient very weary. 11 A.M.—Chloral 3ss., same dose repeated every half hour. 12 noon.—A double dose. 12.30 P.M.—A single dose. 12.45.—A single dose, 3iii. in all. Up to this time she had been conscious, even during the intervals of pain, getting off her bed during the pains, talking rationally, feeling sleepy, but unable to sleep. The pains recur every seven minutes, are sometimes intercurrent, and of long duration. She was ordered to keep to the bed during the pains; and when examined, the os was found still in the same condition. 12.55.—Perfect repose; pulse soft, 88; respirations 18, very easy; pupils normal. She remained for half an hour asleep in a state of the most perfect repose. 1.30.—Believing the anæsthesia to be sufficient, I asked Mr Hinchcliffe, who was present, to examine her; he found the os dilated to size of half-a-crown, and brought away some perfectly fluid blood. Dr Keiller had first arrived, and he examined the patient. Unfortunately the first examination had disturbed the rest, and she was now agitated and complaining, rolling her head on the pillow, and asking for the nurse to support her back. She was, however, but partially conscious. By a singular concurrence of circumstances, a patient in labour with her fourth child was at the same time on the second bed of the Delivery Room, just completing the second stage of a painless labour. This and her former labour (3d and 4th) had been without pain, and the contrast was painfully disadvantageous. 15.5.—Chloral 3ss. 2.10.—Excited, talking much. 2.30.—Quiet and apparently unconscious, notwithstanding the disturbance attending the termination of the other labour and the squalling of the child. 2.40.—Again awake; eyes open naturally for the first time since nearly two hours. Asked her whether that child was hers. Said she thought she could not have got over it so easily. Asked her again whether it was her child. She felt her abdomen and said, No. I was unable to watch the case longer. The labour terminated at 8.30 P.M. Os remained

as above till 5 P.M., when it began to dilate, and second stage was only one hour.

CASE X.—Jessie R., aged 24; second pregnancy; 19 Vennel. Had her first child at the Edinburgh Maternity, three years ago, under choloroform.

18*th February* 1870.—On the present occasion was in labour since 12 P.M. 4 P.M.—Head low in cavity; membranes entire; pains frequent and strong. Ruptured the membranes. Patient very weary, very excitable, crying for choloroform. 4.10.—Gave chloral 3i., and told her it was the new way of giving chloroform. 4.20.—Head was advancing, and anæsthesia not marked; gave chloral 3i. She almost immediately ceased to manifest any excitement; remained quite quiet, and complained no more, not even when the head passed out at 4.40. Twenty-five minutes later, the placenta being still retained in utero, I introduced the hand, and found a portion of it grasped tightly at the left superior angle; withdrawing it, I failed to remove a portion of membranes, and re-introduced the hand, which had to contend with the same cause of retention. Patient perfectly unconscious. About forty minutes after the birth of the child, she began to speak from time to time, and soon opened her eyes. The noise of the child crying prevented her settling down to rest; but she was ignorant of the birth of the infant, and frequently asked "whether she was better—whether she would soon be better." She never inquired whether that child was hers. I had put the binder on over the naked abdomen, and she several times declared that somebody must have carried away her shift.

She now began to talk of her mother, who died last August, as if alive, and wondered when she would come. Now and then she slept for a minute or two, and then began again to rave; she talked rapidly, and often changed her subject. She said the doctor had tried to persuade her that was the new way of taking chloroform. "The doctor cheated me—I am sleeping, but I don't know myself—I wish I could sleep; I am very tired"

(with her eyes shut). "I have tasted that stuff, I wonder what it was—I don't know, but it was not chloroform." Repeated often, "Am I better?" 5.25. —Began to remember that her mother was dead, but on Dr Niven coming in, asked when her mother would be back. Left her at 5.45. 8 P.M.—Found her asleep, and was told she had been talking a great deal of nonsense.

19th Feb., 10 A.M.—Did not sleep well on account of a cough she had had some time past, otherwise quite well. Remembers the birth of the child — does not remember the removal of the after-birth. Says that the new chloroform is longer putting you over, but that there is not the painful whirring sensation in the head.

CASE XI.—Mrs X., aged 23; primipara; in good health, but much troubled with nervous irritability during her pregnancy. Her dread of pain exceeds any powers of description. Her labour began just after midnight, 9th April. When she could no longer doubt that she was in labour, her excitement and irritability became great. Chloral was administered in ʒss. doses every half-hour from 1.30 to 2.30 A.M. The effect was manifest by the time the second dose was taken, but it seemed desirable to continue the administration, in order to remove some remaining indications of restlessness. From this time, 2.30 to 8 A.M., the pains continued regular and strong; the patient quiet, and perfectly unconscious. An examination made at 6 A.M. showed the os fully dilated, and everything normal. The bladder was emptied with the catheter. At 8, contractions began to flag, and before 10 I wrote to Dr Keiller, advising him of the state of the case. He was in attendance immediately, and examined the patient. Condition of perfect quiet; pupils rather large, but perfectly contractile. No signs of suffering on the part of mother or child. Dr Keiller decided to wait. Labour made no advance. The head remained as above, in good position, but in the middle of the cavity. Towards 2 P.M. the patient became restless, and would not lie long in any position. Rolled the head from side to

side frequently; no speech. At 3 P.M., Dr Keiller delivered with forceps a male child, which was with difficulty resuscitated, and which remained for twenty-four hours very weakly. Chloroform was administered at the last stage of labour, and its effect was, on the one hand, to quiet the restless agitation, and, on the other, to restore speech, and with this singular result, that the mental preoccupation, which one would have supposed effaced, was revealed as intense as ever; the few words she spoke having reference to her one extravagant preoccupation—her labour. The placenta was expelled naturally within ten minutes. She took food that evening, and slept soundly till next morning, feeling perfectly well, but *hurt*—so that here again her nervousness intervened to retard her recovery; for some slight pain about the parts, when attempting to pass water, alarmed her, and she maintained a voluntary paralysis of the bladder for three weeks, and was only cured of it by the exercise of moral severity. The passage of the catheter left behind much irritation of the bladder. The convalescence was interrupted by an attack of fever — thirty-six hours' duration — on the twelfth day. She gave up nursing five weeks after her confinement; but improper management of the child, from the twelfth to the fifteenth day, must be considered the chief determining cause of this change.

When we are considering the application of chloral to the suppression of pain during labour, the point of primary importance is to determine the influence of the agent on the uterine contractions.

Before proceeding further, I will reserve Cases I., IV., VI., VII., VIII., XI., for future discussion, and confine my remarks for the moment to Cases II., III., V., IX., X., which formed the groundwork of my experimentation.

In Cases II., V., X., the second stage of labour was well advanced, and anæsthesia was as complete as could be desired. Cases III., IX., are severally good types of the two classes of patients we meet with in practice under circumstances of protracted suffering, more especially, as

in both these instances, there was a tedious first stage. When labour has been protracted until we observe marked distress on the part of the patient, the prominent symptoms are either unnatural excitement or great depression; the pains are irregular, often intercurrent, the intervals not distinctly marked; and who has not heard the patient imploring "a wee sleep?"

In the condition of extreme torture, observed in Case III., the effect of chloral is strikingly exhibited in the case as recorded.

The power of the medicine in diminishing pain was well instanced in this case, when the uterine contractions, tested by the hand on the abdomen, began fifteen or twenty seconds before the patient exhibited any consciousness of pain, which consciousness was again lost before the contraction had expended itself. Intercurrent pains disappeared entirely, and the uterus contracted with a perfectly regular periodicity in periods of longer duration than formerly. Intensity and duration cannot but be a test of efficiency, and my conviction was that the contractions were more efficient. Pain was not superseded, because my experience was not sufficient to allow the exhibition of fuller doses; but in a like case, with much marked reflex excitability, I should like to try conjointly with chloral a very moderate exhibition of chloroform. I should prefer this to increasing the dose of chloral.

The class of patients in whom depression is observed is well exemplified in Case IX. The effect was so marked that the phenomena of labour might have been thought to be suspended. Perfectly painless contractions recurred at very regular intervals, and so effective were they that, in the short space of half-an-hour, the os dilated more than during the preceding wearisome eight hours. Dilatation was so rapid that the structure bled. Acting upon the conclusions drawn from the observations I could command, I secured to a patient, whose nervousness exceeds anything I ever observed, a perfectly painless labour in Case XI.

I now return to the six Cases I reserved—I., IV., VI., VII., VIII., XI.

CASE I. is an instance of idiosyncrasy—absolute intolerance of the medicine. The results obtained in the four remaining cases are not less interesting that they are negative. In all, ergot was administered conjointly with chloral; and although I confess that the experiments are far too feeble to establish a point of such paramount interest, still I cannot but draw your attention to a subject which observation will readily decide. Is chloral antagonistic to ergot? If so, the therapeutic importance of the fact would hardly outweigh its physiological interest. Dr Liebreich, who introduced chloral to the profession, followed up his researches by the discovery of its antagonism to strychnine; and if my inference is correct, we shall have the two great tetanizing agents, each in its distinct sphere, affecting a like antagonism to chloral.*

I hope I may now be permitted to make a few observations on each case, *seriatim*, in order to point the interest of each.

CASE I.—Intolerance of medicine.

CASE II.—A very quiet nature of woman, devoid of excitability. Result very favourable. The peculiarity of this case, the first we witnessed, was that the patient several times, during the apparent condition of unconsciousness, answered questions pertinently; in fact, woke out of her sleep to answer them, and fell off at once again. More—she appeared conscious of some pains, and especially of the birth of the head; but the memory of the whole circumstances was effaced when she woke five hours later, and it remained so.

CASE III.—A strong, powerful, muscular woman, who came up from the country twelve hours after her labour began, and wandered for a long time in Edinburgh, not knowing where to go. The case has received sufficient comment previously.†

* The physiology of this statement is obviously incorrect; the observations are probably defective.—ED. *Edin. Medical Journal.*

† See *ante*, p. 161.

CASE IV.—Ergot administered before chloral, negative result; other causes tending to negative the hypnotic influence will be referred to further on.

CASE V.—A very remarkable case, quite the parallel of Case II., but complicated with slight cerebral disturbance. Dose too powerful. The girl owed her recollection of the termination of labour to the incident of the commotion produced around her by the resuscitation of the infant.

CASE VI.—Chloral would not have been administered if I could have foreseen the course of labour. This case will be referred to again.

CASE VII.—Ergot administered conjointly with chloral. This case especially suggests antagonism.

CASE VIII.—Marked cerebral disturbance for a short period. This case will also be the subject of critical remarks.

CASE IX.—Perfect anæsthesia for the space of half an hour, interrupted by a vaginal examination.

CASE X.—Very favourable result marred by the occurrence of sub-delirium, more than half an hour after delivery. Hand introduced into uterus, and placenta extracted without any resistance on the part of the patient.

CASE XI.—Painless labour throughout. Was chloral the cause of the cessation of labour-pains, of uterine contraction? They had continued very efficient for four hours, and the os was fully dilated. They continued two hours more without producing any effect on the head. Is it just to incriminate chloral when we want any correlative proof that it has any tendency to affect injuriously uterine contraction? Before offering any conclusions, I have to add a few remarks on the administration of chloral, presuming that their importance is only secondary in place and not in degree. Sir James Simpson insisted strongly on the influence of external impressions upon the character of the anæsthesia under chloroform, and his rules should be more stringently enforced in the exhibition of an anæsthesia of less potency. What we aim at with any medicine is the attainment of a desired

end, with the use of the smallest possible quantity. This is especially urgent with a remedy administered by the mouth. Chloral, too, has a very complex action; capable of suppressing reflex action and also consciousness, it seems, *when given in excess*, occasionally to exercise a stimulant action on the cerebro-spinal system, producing disagreeable effects, not compatible with its introduction to private practice; effects, however, be it said, transient and never dangerous.

On this account it is imperative that the patient be surrounded by conditions tending to perfect repose of mind and body; and when there has been great previous excitement, we must proceed very cautiously. For the attainment of so delicate a result as anæsthesia—"Thus far and no farther"—great care is required, and we must do with chloral as we do with chloroform, administer it tentatively by fractional doses. I have sat for two hours by a pregnant patient brought into the Maternity in the middle of the night, in consequence of severe vomiting; and I have given chloral in successive small doses, watching the case until I knew that perfect rest had been induced; and the next morning, after sound sleep, the cure was perfect. The first successful case I saw treated by Sir James Simpson (Case II.) was a type of good practice, but it was favoured, as no other case has been since, by the general condition under which it occurred—"a quiet nature of woman," I have said, "devoid of excitability—a private patient in the hospital, not surrounded by half-a-dozen tattling drones, or curious mutes. Perfect quiet; night time. Case VIII. offers an exact counterpart of this; the conditions there were most unfavourable for the trial.

It is only when one has observed that one knows the counteracting influence of small things; and though I sincerely believe that the future of painless labour will not rest with chloral, but that science will discover other remedies more applicable still, I am at present only engaged in the chloral experiment, and it is my duty to

insist upon the conditions of experimentation. Those who, with an honest conviction, say I have never seen such effects, must show henceforth that they have fairly experimented.

In the homes of our Scotch labouring classes, the chamber of a parturient woman is the resort of all the gossips who can gain admittance; whilst the absence of almost every comfort is rarely pushed to the extreme limit of abstinence from stimulants. I made mistakes when I began to experiment, and this warning, if necessary, will prevent their repetition. Cases IV. and VI. were especially refractory, from causes above mentioned. The production of anæsthesia is favoured by all that lulls the senses. I have failed at the Maternity to quiet a wilfully noisy, garrulous patient, even with chloroform, given for nearly an hour to the extent which is permissible in parturition. Chloral given in massive doses to an excited patient, whilst it stupifies consciousness, intensifies reflex irritability—Case VIII.;—or where it quiets reflex action, may awake unconscious cerebral activity—Case X. Persons present at the administration of anæsthetics in midwifery should therefore be competent to understand suggestions or obey instructions.

Noise must be suppressed; no talking on the part of the patient encouraged; her movements controlled without violence. Examinations and any operative interference should be avoided.

When anæsthesia has been complete and prolonged for some hours, the bladder must be emptied; and it would be advisable, when using the catheter or making a vaginal examination, to avoid the induction of reflex action by giving a few whiffs of chloroform.

In conclusion, I appeal to your indulgence against the possible enthusiasm which has extended this communication beyond reasonable limits.

Conclusions.—1. Chloral is an agent of great value in the relief of pain during parturition.

2. It may be administered under favourable circum-

stances during and at the close of the second stage, with the result of producing absolute unconsciousness in the same sense in which we understand unconsciousness under chloroform.

3. When thus given successfully, it has this advantage over chloroform, that it requires no interference with the patient.

4. It is desirable to retain chloroform in the position which it at present occupies in midwifery, and to reserve for the agency of chloral the first stage of labour. If, however, chloral or some agent having analogous properties is found successfully to relieve the pain of uterine contraction, the use of chloroform will be restricted to a lesser period of the duration of labour, or to the facilitation of manual or instrumental interference.

5. It is demonstrated that a labour can be conducted from its commencement to its termination, without any consciousness on the part of the patient, under the sole influence of chloral.

6. The exhibition of chloral in nowise interferes with the exhibition of chloroform.

7. The proper mode of exhibiting chloral is in fractional doses of grs. xv. every quarter of an hour until some effect is produced; and according to the nature of that effect the further administration is to be regulated. Some patients will require doses of ʒi.; and it is better to produce an anæsthetic effect by ʒiii. given in the space of two hours than by ʒi. given singly.

8. The effects of chloral are continued beyond the period of completed parturition, and the repose experienced by the patient after her labour is one of the favourable circumstances to be noted in considering its application to childbirth.

9. Any stimulating effects, in the form of general excitability, occasionally observed during the administration, have passed away very rapidly.

10. Chloral not only does not suspend, but rather promotes uterine contraction by suspending all reflex

actions which tend to counteract the incitability of the centres of organic motion.

11. Labours under chloral will probably be found to be of shorter duration than when natural, for unconscious contractions appear to have more potent effects than those which are accompanied by sensation of pain.

12. Experiments are required in order to determine whether there exists the same antagonism between ergot and chloral as is known to exist between strychina and chloral.

13. The general conditions under which chloral is to be administered are the same as those which regulate the administration of chloroform, and the rules laid down by Sir James Simpson in connexion with this subject must be rigidly adhered to.

The discussion on Dr Cairn's paper was now proceeded with in connection with the preceding.

Dr Burns said that he had had but little experience of chloral, and that had not proved very satisfactory. In one case of delirium tremens, the second or third dose made the patient sleep for three hours, but the remainder of them did not do him any good. In a second case, a gentleman asked him if he might take it. The first dose rendered him perfectly excited. In two days afterwards, he was called to see the same person. He had taken a dose the night before, and had slept from 10 P.M. to 10 A.M. It was thought he had taken a fit. On my arrival he had much recovered; he had congestion and blueness of the lips and face. He forbade any more chloral being given. In another case in which it was given, it had no effect whatever. His experience, however, was so limited, that he could not give a very definite opinion. In reference to the cases recorded in the paper just read, he considered that they threw very little light on the subject. All the labours mentioned had been very protracted.

Dr Thomson had given it in a case of hysterical neuralgia. In one case he saw, it produced effects so very similar to those of alcoholic intoxication, that it was only from the strong assurance of the medical man in attendance that she had got nothing but chloral, that he was enabled

to form the idea that they did not arise from stimulants. In another case, no rest could be got at night without the chloral. In the case of this patient, he recommended her removal from her family. In another case of neuralgia of the left breast, many things were tried; she had large doses of opiates and bromide of potassium without avail; 20 grains of chloral, or 6 grains at bed-time, had given her a most excellent sleep. Afterwards, when the pain again resumed its former severe character, he thought that some internal organic disease might be present; and accordingly, after death, he found a tumour in the thorax and observed caries of the ribs posteriorly, about the centre of the back. The neuralgia was caused by the disease of the bone. There was also inability to pass urine; very much to his annoyance, as it had to be removed. He felt disinclined to use it in ordinary cases of parturition—he preferred chloroform; besides, it was a depressing agent, from which the patient did not readily recover.

Dr James Young said that he had used chloral largely, and found it most serviceable as a hypnotic, inducing sleep and relieving pain, in doses of half a drachm. In delirium tremens it was especially beneficial. In the case of a gentleman in Princes Street, whom he had seen lately, it was of the greatest use. The patient was violent and unreasonable, and had slept none for several nights. He was induced to go to bed, and had one drachm of the hydrate of chloral administered. He slept several hours, and awoke comparatively well. Next night he had another drachm, and on the following day went to his business. In cases of severe nervous headache, he had seen quiet sleep, with relief, produced by drachm doses. He had a lady patient residing in France, who wrote him that she could get no sleep in consequence of intense headache. He sent her a prescription for chloral, and by a letter he had received, she stated that the relief derived was inconceivable. Dr Young said he had used chloral in cases of infantile convulsions in doses of one and two grains, with marked benefit in checking the convulsions and inducing sleep. In cancer, he had used it frequently with good results.

Dr Cairns made some remarks in reply to observations on his paper.

CASE OF HYDROPS OVARII, WITH ASCITES AND HYDROTHORAX.

By W. Marsden, M.A., M.D., Quebec.

June 22, 1870.

"Mr President and Gentlemen,—Having read the report of the Proceedings of the Obstetrical Society of Edinburgh, of the 6th of January last, in the *Edinburgh Medical Journal* for March, page 849, I am induced to submit the following case from my case book (taken nearly a quarter of a century since), under the impression that it may be interesting to your Society, and at the same time that it may tend to elucidate some pathological theories propounded, whilst it may confirm others, in the case of Miss M. O., presented by Dr R. Peel Ritchie."

(I was requested by the friends of the deceased to make a *post mortem* examination of the body, which I did, assisted by Dr J. O. Rousseau, the attending physician to the deceased, and Dr P. Brassard.)

Josephte Bonneville, widow of Joseph Lemire, of St Esprit, in the district of Three Rivers, died on the 24th of February 1847, æt. 54. On the 25th, twenty-four hours after death, made the *post mortem* examination. The external dimensions of the body were as follows, notwithstanding there was great emaciation, and a considerable discharge of fluid had taken place, completely saturating the bedding and floor:—Circumference of the body, over the umbilicus, fifty-eight inches; the circumference of the tumour, horizontally round the scrobiculus cordis sides of the abdomen and pubis, sixty-one inches; perpendicularly from the sternum to the pubis, thirty-three inches and a half.

On opening the cavity of the abdomen, it was found to contain an enlarged ovarium, completely displacing all the other viscera. The diseased ovarium was of the

left side, and was adherent to the peritoneum throughout the greater part of its extent; and to the diaphragm, transverse colon and bladder. The ensiform cartilage stood nearly at right angles with the sternum; and when the tumour was removed from the body, the inferior margin of the ribs gaped like the mouth of a funnel. A line drawn from the anterior inferior angle of the ribs to the spinal column measured eleven and a half inches. On removing the tumour from the body, it was found to weigh seventy-one pounds. It was of the encysted multilocular variety, and divided into about eleven principal compartments, which were again sub-divided into many smaller ones, and contained fluids of different kinds. One large one contained upwards of two pounds of scrofulous-looking matter; others contained a dark yellowish-brown pus, mixed with albumen; and others again a ropy-kind of mucus and dark serum. The tumour or carneous mass, when emptied as far as practicable by puncturing the cavities, weighed forty-five pounds. The abdominal cavity contained a large number of hydatids, varying in size from a few drops to two ounces, which were attached by long pendulous necks to such portions of the peritoneum as were not involved in the tumour. In shape they resembled a Florence flask, or rather a Riga balsam bottle, somewhat flattened on one side, and broader at the base than the shoulders, but much longer in the neck, some of the smaller ones being five or six times as long as the body. The hydropic tendency was general, as that portion of the cavity of the abdomen, which was unoccupied by the tumour, contained five and a half gallons of highly-coloured serum. The cavity of the thorax also contained six pints of bright-yellowish green serum. The bladder was empty, and its walls extremely thin, and flattened out to a diameter of about eleven inches. The great omentum was so completely involved in the diseased ovarium as scarcely to be distinguished, excepting as forming a band of union with the transverse colon. The uterus was elongated and fusiform,

the point being towards the fundus uteri, and about five inches long, and the broadest part about an inch in diameter, near the cervix uteri. Could distinguish remains of the right ovarium, but no traces of the Fallopian tube of that side. The left Fallopian tube was considerably elongated, widened, and ramified over the side of the tumour. The concave surface of the stomach and the duodenum and jejunum bore inflammatory traces, and especially the latter. The right kidney was little more than half its natural size, but the left one was of the usual size, and healthy. The spleen was enlarged and unusually light coloured, being nearly the colour of the liver. The mesenteric glands were enlarged and schirrous. All the intestinal canal, excepting the portion above described, healthy and empty, excepting the rectum, which contained a quantity of greyish-looking, indurated fæces. The liver remarkably healthy looking, and the gall bladder empty. The lungs healthy looking, but the left one had several adhesions *to the diaphragm.*

The subject of this disease was five feet seven inches high, of bilious, nervous temperament, of excellent habit of body, and had not been subject to disease of any kind until after the cessation of the catamenia. She is the mother of one child—a son, now 34 years of age. Her own history of her case is as follows :—She married young, menstruated regularly and early, although sometimes copiously, until she was 45 years of age. Had only one child, and never miscarried. Her general health continued good for some months after the catamenia had ceased, when she was attacked with dysuria, with pains in the back and loins, as when the menses were going to flow. At the end of two years, imagining that such was about to be the case, and that if so, she should enjoy better health, she placed herself in the hands of an empiric, who guaranteed that he would restore the secretion, and under his treatment had one change, which lasted several days. After this she began gradually to increase in size, and her husband being then alive, she imagined, until

upwards of nine months had elapsed, that she was pregnant.

Her general health, notwithstanding the very great size she had attained, continued almost unimpaired, until the autumn, when her appetite began to fail, and she visibly lost strength and flesh, but the animal spirits were, until within a few weeks of her decease, uniformly buoyant or cheerful. At that time (the autumn) she began to find her size inconvenient, though not painful. Previous to that she had continued to walk out alone; subsequently, however, she became subject to constipation, irritable bladder, etc. Until four weeks before her decease she walked about without help, and on the Sunday previous to her decease, she walked about her room with a person to assist her.

A singular fact connected with this case is that *she leaves a sister, two years older than herself, who is similarly afflicted; and a maternal aunt who has been labouring under an enormous abdominal enlargement* for fifteen or sixteen years past.

A remarkable feature in this case was the very slight constitutional derangement, notwithstanding the long existence of so enormous a tumour, which looked like a huge cone. The whole weight of the morbid accumulation in the abdominal and thoracic cavities, could not have been less than 130 lbs. Thus—

The ovarian tumour, ...	71 lbs.
5½ gallons of serum in the abdominal cavity,	44 „
6 pints of serum in the thoracic cavity, .	6 „
10 pints by escape after death, say, . .	10 „

The escape from the body, after death, could not have been less than one and a-half gallon, as the abdomen during life, and immediately after death, was hard and tense; and when examined the day after, it had become comparatively soft and flaccid, so that we could readily distinguish the character of the tumour through the abdominal walls.

I will merely make one or two remarks on this case, leaving it to your society to draw its own conclusions.

1*st.* Dr Ritchie observed that "he also believed that a hereditary tendency existed" in his case.

It will be seen that the hereditary tendency was well marked in the case of Madame Lemire, extending to an elder sister and her maternal aunt. I have since seen and acquired the history of two other cases showing the hereditary tendency. In one case the subjects were two sisters, and in the other a mother and daughter.

2*d.* "Dr Ritchie thought it was possible, and he might even say probable, that an endosmotic change had taken place by means of the large vessels to which allusion was made among the *post mortem* appearances."

While I freely admit the possibility of endosmotic action, I cannot avoid leaning to the suggestive interrogatory of Dr Bell, "whether the previous history of the case might not lead to the supposition that the increase and decrease of size was dependent on ascites?" although "no fluid was found on examination in the peritoneal cavity."

Dr Ritchie says: "On the 4th of September it is stated she passed urine involuntarily as she sat about; and on the 9th of same month, that for the last two nights she had passed immense quantities of water. In consequence she was much reduced in size. On the 19th she was reduced to 41; and by the 19th October she had diminished to 38 inches." This establishes Dr Bell's theory of ascites conclusively in my mind.

In conclusion—it is worthy of record, and due to the memory of the deceased, to state that it was by her own express desire, frequently reiterated to the writer, that a *post mortem* examination of her body took place; and it is still more creditable to her feelings, when it is considered that the greatest antipathy and prejudice against *post mortem* examinations exists among the peasantry in Canada; and that the majority of her friends and relations were so much opposed to it, that nothing but the fear of disinterment of her body could have induced them to accede to her request.

Dr Ritchie observed the Society was obliged to Dr Ramsden for having sent his interesting case, and personally he was so to him for having taken so much notice of the case he had laid before the Society. It, however, differed in most particulars from that of Dr Ramsden's

As regards the hereditary tendency in Miss M. O's case, the remark had reference to the insanity, not to the ovarian disease, and Dr Ramsden must have misunderstood his description as to the alteration in Miss M. O's size being due to ascites; he was of opinion that seeing her size at the time of her death was increasing, fluid in the abdominal cavity would have then been surely found if the previous increase and decrease in her size was to be explained by its presence or absence. The absence of peritoneal fluid was the great difficulty in accounting for the alterations in her size.

CASE OF DECAPITATION AND EVISCERATION, WITH ADHERENT PLACENTA.

By Thomas Cairns, M.D.

June 22, 1870.

The subject of this case is 33 years of age, and the mother of six living children. She has always been attended by a midwife, and has always got over her confinement with great ease. Generally speaking, she has always enjoyed remarkably good health. At six o'clock A.M., seven weeks ago, I was sent for, and urgently requested to come immediately, "because the patient was in great danger." On my arrival, which was in the space of fifteen minutes after the message had been sent, I found the midwife sitting very contentedly by the fireside smoking a pipe. After remarking that nothing very serious could surely be wrong when the accoucheur was taking matters so coolly, I was about to leave the house forthwith, but was induced to remain at the earnest entreaties of the patient's friends. On examining the

patient, I found the head completely expelled, in size averaging nine months gestation, of a dark purple colour, of low temperature, and with entire absence of pulsation in the fontanelles. Stethoscopy elicited *no sound of the fœtal heart*, but a *very distinct bruit*, corresponding to the right side of the uterus. Immediately after this examination, I learned on inquiry that the patient had "run her full time," that she had suffered acute pain in her right side for about three months; that labour had begun about six hours before I was summoned, and that the case seemed to go on very favourably for about four hours, when the *pains entirely ceased*. By this time there had been no pains for two hours, during which time the head had been "born." Ergot of rye and other uterine stimulants hereupon suggested themselves, but having great faith in nature's "hints," I immediately concluded that something very material was wrong, and proceeded accordingly to assist nature by manual, not by medicinal agency. And here I must make a correction in regard to the heading of this paper. I have called it a case of decapitation, because I have not been able to find any other more appropriate term. The head, however, was not in this case cut off, it was wrenched off, and along with the right arm. Finding the child perfectly dead, I felt no scruple in using the utmost liberty with it, so as to secure its separation from the mother, and with the latter's safety. After, therefore, trying simple traction without avail, I twisted the head round about, at the same applying a certain degree of direct tractive force, when, after little effort, the head and the right arm were separated from the trunk. After giving the patient five minutes rest and a large dose of sherry, I introduced my hand into the uterus, and found the abdomen of the fœtus enormously distended. I thereupon opened the child's belly with scissors, when there followed a large gush of bloody serum. Still retaining my right hand in the uterus, and after administering another dose of sherry, I secured a firm hold of the fœtus, and proceeded to

extract it, but without the least avail. I then put my hand right into the child's abdomen, and finding a kidney unusually large, as I thought, I removed it, along with, I think, part of the other viscera. I tried traction once more, but with no better result than on the first occasion. I gave the patient another rest of five minutes, and a very large dose of sherry, still keeping in the uterus my hand, which I then passed *over* and *around* the body of the child, whereupon I found two cords warped round the body, close to the flexure of the thighs, and the placenta firmly glued to the right side of the uterus to the extent of half its diameter. On finding this state of matters, my first object was to set free the fœtus. With this view, I at once ruptured the cord, which happened to separate at two points. No hæmorrhage ensued, and the fœtus was extracted without the least difficulty. After an interval of half an hour, and the administration of another dose of sherry, I removed the placenta, when the uterus contracted exactly as in a case of natural labour. After remaining with the patient for about an hour, and finding no unfavourable symptoms presenting themselves, I left her in charge of the midwife, with all the directions which I considered necessary in such a case.

Without entering further into details, suffice it to say, that the patient made such a good recovery, that, when I paid her my last visit, which was on the tenth day after her confinement, I found her at the "washing tub." I have not seen her since. The entire uterine contents of this case I handed over to Dr Keiller, who I have no doubt will be glad to shew them to any Member of the Society who may wish to see them. In relation to this case, I have to observe—

1*st*. That it was only yesterday that I learned from a neighbour of the patient the probable cause both of the death of the fœtus and the adherence of the placenta. The cause alluded to was this. The patient's husband, about two months before the birth of the child, when in a state of intoxication, "dragged her by the hair along the floor, and gave her several kicks in the belly with his tacketty

shoe,"—a circumstance quite sufficient to account for the condition of the child, and that of the after-birth, as above described.

2*d*. The second point to which I beg to call attention is, the sudden and continued cessation of the uterine pains. And to this observation I crave the special notice of the younger Members of the Society, because, be the reason what it may, I do not find this matter even hinted at in any one of our standard obstetrical works. Why, in such a case as this, and in other similar cases, do the pains abruptly stop, and either do not recur at all, or at very, very long intervals? The answer, in my opinion is this,—*Nature knows her own powers.* She puts forth her best efforts, and sustains them so long as she thinks they are likely to be of use. The moment these efforts prove abortive she becomes quiescent—*she does nothing, absolutely nothing;* and *then* is the time to determine whether she should be left to her own unaided efforts, or whether she should receive extraneous help. In the case under consideration, the natural efforts of the uterus had become utterly effete by the time I saw the patient; and to have administered ergot of rye, or used other means to excite uterine action, would not only have done no good, but, in all probability, an immense deal of harm. Following, however, as I did the hint which nature threw out, the very best result ensued,—the patient, as I have mentioned, having made a most excellent recovery.

3*d*. The only other point to which I shall call attention is this, that, although no uterine pains had occurred for a couple of hours before I removed the uterine contents, the moment the uterus was emptied, it contracted just as fully as it usually does in a case of natural labour.

ON SOME OF THE DANGERS ATTENDING THE USE OF TANGLE TENTS.

By Lauchlan Aitken, M.D.

July 13, 1870.

My attention has frequently been called by students and others to the almost complete silence of gynækological writers on the subject of any dangers attendant on the dilatation of the uterus by tents of various descriptions, and, on looking over my case-books, I have been struck myself by the number of accidents and complications ensuing, especially from the use of the tents made from the sea-tangle. In order to put my professional brethren on their guard, and to ensure the requisite amount of caution in the employment of this valuable method of uterine diagnosis, I determined some time ago to bring the subject before the Obstetrical Society of Edinburgh in a short paper, but this idea circumstances compelled me to abandon at the time. Having lately, however, had some conversation on the subject with an esteemed writer on similar topics, I was led by his remarks again carefully to collect the facts on which I relied for my own opinion on the matter; and the data thus put together constitute the essential features of the present article. I shall restrict myself, in the meantime, to those injurious results I have seen follow the use of the sea-tangle tents, reserving any observations I have to make on the other modes of uterine dilatation for another communication.

Before entering upon the subject-matter of the paper, however, I cannot forbear reminding you that the idea of dilating the womb for the purposes of diagnosis and treatment was one of the many products of the fertile genius of my late friend and master, Sir James Simpson; and this very invention, along with that of the proper use of the uterine sound, would in themselves have succeeded in making him famous as a gynækologist, even if he had never written another word on the diagnosis or treatment of the diseases of women. It was as early as

the year 1844 that Sir James Simpson first wrote an article on the subject of sponge tents in the *Monthly Journal of Medical Science.*

The sea-tangle, *Laminaria digitata*, was only recommended in the year 1862* by Dr Sloan of Ayr, as a substance which, when dried, expanded greatly after its immersion in fluids, and it was principally owing to the influence and example of Professor Simpson that it came to be extensively used by the profession generally as a convenient means of dilating the cervix. Of late years it has somewhat fallen into disuse, mainly owing to the fact that sponge tents are now much better prepared, and can be had of smaller diameter than formerly, and partly also by both sponge and tangle tents having been to a slight extent superseded by the dilating bags which go under the name of Dr Barnes.† My own opinion, however, is still strongly in favour of the use of the sea-tangle in those patients in whom the cervix is very narrow, or in whom there is some stricture. In fact, it is only the smallest variety of tent that can then be introduced, so that we are frequently compelled, in such a case, to accept the tangle tent as an unavoidable necessity.

That I have no desire to exaggerate the importance of this subject will, I hope, appear from the following observations; but I must first direct attention to the remarks made on the matter by the authors who have written the most recent gynæcological text-books in this and other countries. To take one English author, for example, Dr Graily Hewitt, in the second edition of his work on the Diseases of Women, makes little or no reference to the occasional bad effects of dilatation of the cervix in those paragraphs in which he speaks of tents, and only incidently mentions, in treating of cellulitis, that their employment may occasionally give rise to that form of pelvic inflammation. Churchill, so far as I know,

* *Glasgow Medical Journal*, October 1862.

† The original suggestion of elastic dilating bags was undoubtedly made by Dr Keiller of Edinburgh.

does not once allude to the subject in his important and well-known work, and of West's book no edition has been published since 1864, when the tents of laminaria were necessarily only imperfectly known. Of foreign authors, Courty of Montpellier, in his "Traité Pratique des Maladies de l'Utérus," also passes over the dangers attending cervical dilatation almost without remark, and even ventures on the assertion, that it is perfectly safe if the dilatation be not conducted too rapidly. Scanzoni,* too, whom we may look upon as the representative of German progress in this department of medicine, while lauding the advantages of the laminaria—its cleanliness, facility of application, and its equal dilating qualities—makes no mention of any inconveniences attending its employment, except a casual reference, at page 382, to a case of pelvic abscess produced by the use of two tents in succession, and which seems to have been under treatment at the time he was writing.

While our ordinary English and some foreign text-books thus almost ignore the existence of the dangers I shall subsequently allude to, it must be confessed that other authors, greatly to their credit, have not been so reticent. Thus, in the treatise On Female Diseases, by Dr Thomas of New York, published at Philadelphia in 1868, mention is made of a fatal case of tetanus following the employment of two tents, and the author also alludes to other severe complications he has had occasion to notice. In the second edition of Nonat's well-known work,† he expresses himself with great reserve on this subject, which he speaks of under the name of soft dilatation—dilatation molle, and enters minutely into the cautions required, and the methods to be adopted to avoid the many inconveniences and dangers to which the tents may give rise. He does not, however, ever seem himself to have experimented with the laminaria.

* Lehrbuch d. Krankheiten d. weiblichen Sexualorgane.

† Traité Pratique des Malades de l' Utérus de ses Annexes, et des Organes Génitaux Externes. Paris, 1869.

In leaving those preliminary remarks, and now entering upon the real subject of this paper, I may point out that what has seemed to me the most common of all the evil consequences arising from the use of the tents, has been the development of inflammatory action in the pelvic serous membrane or cellular tissue; next, the production of endometritis; and, finally, the causation of a number of what may be looked on as accidental complications,—such as hæmorrhage from the uterus, pelvic hæmatocele, laceration of the cervical wall, spasmodic contractions of uterine muscular tissue, hysterical convulsions, and other minor mishaps.* But though many of those complications have been very alarming at the time, and productive of anything but good to the patient, I have never known a fatal case in which I could directly attribute the death to the use of the tent, although I have certainly seen patients succumb to the effects of uterine operations for the performance of which the cervical cavity had necessarily been enlarged.

In a former communication† to this Society I alluded to the difficulty of determining the manner in which the traumatic forms of pelvic peritonitis and cellulitis originated. In bringing before you now a few cases in which the tangle tent was obviously the cause of the attack, I must still continue to profess my ignorance of the way in which the inflammation is lighted up. There can be no doubt that endometritis exists in many of those cases, but I still hesitate to accept its progression along the mucous membrane of the Fallopian tubes as an escape from our difficulty, and I must retain this opinion until post-mortem examination has satisfied myself or others of the reality of such an occurrence. My own observations, too, have never convinced me that a genuine *metritis* was in any case the precursor of an attack of traumatic pelvic peritonitis or cellulitis.

* Pyæmia has also been mentioned as having been observed to follow the use of tents, but I have only given those complications which have come under my own observation.

† On Pelvic Peritonitis and Pelvic Cellulitis, with Illustrative Cases.—See *ante*, page 77.

Among several cases of pelvic peritonitis which I have seen follow the use of the laminaria tents, the following is an example of the almost astonishing rapidity with which in many instances such an attack is developed, and of the equally surprising quickness with which the inflammatory products frequently disappear:—

Jessie C., æt. 29, admitted to Ward XII., Royal Infirmary, 10th Feb. 1868. Patient has been married about two years. Two months after marriage she had an abortion, and has never menstruated regularly since, usually only every five or six weeks, and always with great pain. She is generally very sick before the periods. Has also pain in passing water. Two days after admission, a tolerably thick tangle tent was introduced into the cervix without difficulty, and was removed at night. The next day she complained of great pain in the pelvis, and, on pressure, above the pubes. The pulse was 120, tongue furred and dry. On vaginal examination, on the second day after the tangle tent had been withdrawn, a swelling was found on both sides of the uterus, filling up the pelvis. The womb was fixed all round, and very sensitive to the touch. A good deal of sanguinolent discharge from the os. During the subsequent few days the swelling became harder; but the pain diminishing, and the general symptoms improving, she was put on a tonic treatment, and very rapidly recovered, the inflammatory products having, I find by the case-book, entirely disappeared in less than a month from the date on which the dilatation was effected. On the 19th March she was quite well, and was dismissed.

In this woman I had discovered nothing in the state of the uterus or its appendages to contra-indicate the employment of the tent, which I considered necessary for diagnostic purposes, nor was there any precaution wanting, as I then thought, that ought to be taken in all such cases; but, in spite of these, the limited peritonitis began almost immediately upon the removal of the tent, and was easily detected by vaginal examination two days

afterwards. Fortunately the inflammation ran its course very rapidly, and its products entirely disappeared, leaving the patient little or none the worse; but this is by no means always the case, and the following history shows that we ought not to allow ourselves to be lulled into a false security:—

Harriet N., unmarried, æt. 25, was admitted into Ward XII. on 30th November 1868. She first began to menstruate at the age of eighteen, the periods being always painful, and the discharge often too profuse, but during the last two years the dysmenorrhœa has been getting worse. Some ten days before admission a tangle tent had been introduced and withdrawn, and the patient seems to have acted foolishly, getting out of bed the same day. Some days after this she first felt a pain in the left iliac fossa, and for a few inches above the pubes. She was obliged again to take to bed, and I then saw her, and, detecting the commencement of an attack of pelvic peritonitis, sent her to the Infirmary. On admittance, some hardness could be felt in the left iliac fossa, and there was great tenderness there; a heavy bloody discharge from the vagina. Internally violent pain was elicited by pressing on the left side of the cervix, and the uterus, very painful to the touch, was apparently immobile in the centre of the cavity. She was ordered a milk diet, and small quantities of wine and brandy at frequent intervals, as she seemed very weak. By 11th December the pulse had fallen to 104, and was of fair strength; and it is noted that the pain seemed now more hysterical than real, a light touch eliciting cries of agony, while heavy pressure seemed well borne. About the middle of the month she began to cough, and harsh breathing was heard by the stethoscope at the apex of the left lung. She was then put on tonics, and gradually but slowly regained strength. The next menstrual period passed without much pain or any recurrence of the inflammatory attack; but when she was dismissed in the middle of January there was still considerable uterine

immobility, and the same suspicious sign in the left lung.

This poor girl, through her own imprudence, brought on an attack, which by ending in the fixation of the uterus, must, one should say, rather have tended to aggravate than to relieve the dysmenorrhœa from which she suffered; and, besides, she appears to have shown symptoms of the commencement of some lung mischief when in the ward, although this may only have been an accidental complication. That great care and many precautions are necessary, both on the part of physician and patient, in handling such dangerous tools, must be evident to any one who impartially considers such an unfortunate result; but to this subject I shall presently allude at greater length.

In the meantime, I conclude those remarks on pelvic inflammatory attacks produced by tangle tents, with the details of a case of cellulitis which originated from a like cause.

Jane V., æt. 33, admitted to Ward XII., 28th October 1868, complaining of a feeling of weakness in her right side, which had existed for the preceding nine years, but just before admission had become much worse. She had been married fourteen years, had had nine labours, five of which had been instrumental, and six of the children had been still-born. The last labour, which had been in April 1866, had been attended with such severe flooding that she was unable to rise from bed for three months afterwards. Since that date, menstruation had been very irregular, occurring at intervals of three weeks, a fortnight, or eight days. During menstruation the discharge is very profuse, and sometimes lasts for fourteen days. She also suffers from paroxysms of sharp pain in the left breast, occurring chiefly when the discharge is most profuse, and accompanied by violent palpitations. The menses, which appeared the day after she entered the ward, continued very profusely until the 14th November; and, on vaginal examination after their cessation, nothing

being discovered but an enlarged uterus, it was resolved to dilate the cervix. On 16th November a tangle tent was introduced; on 18th November the cervix was fully dilated, and found to be ulcerated near the internal os, and to this part some solid nitrate of silver was applied the next day. On the 20th, she began to complain of great pain over the lower part of the abdomen, the pulse had risen to 100, and the vagina was abnormally hot. She was ordered some liniment of belladonna an chloroform, hot-water injections, and morphia in form of suppository. By the 25th the pain had materially increased; but it was only on this day that a round tender tumour, about the size of a small orange, was detected in the right iliac region, and apparently adherent to the iliac bones. On the 29th it is noted that this tumour was not so distinct, there being only abnormal dulness on percussion. On the 10th of December there was nothing to be felt in the fossa, but pressure still caused pain, and by the 18th of the same month she was well enough to be out of bed. The treatment was tonic for the most part, with local soothing applications to the painful part.

My reasons for considering this a case of cellulitis have been dwelt upon at some length in the article already quoted, and I need not here enter again into the minutiæ of a differential diagnosis. It was a mild case, presenting no alarming features whatever, and leaving no bad effects behind. Some of you may look upon the nitrate of silver as a more likely cause of the inflammatory attack than the tent; but that caustic was applied so lightly and superficially, that I do not think it could possibly have produced such an injurious effect. Certainly it is true—and this truth ought to be well borne in mind by medical men—that even a trivial application or operation has an element of danger added to it by the previous dilatation of the womb, which is wanting in cases where no such enlargement of the cervical and uterine cavities is required.

To the next branch of my subject I shall only very

briefly refer, as, although, I have often seen endometritis follow the use of the laminaria tents, I have never seen any obstinate cases, or even any which did not yield rapidly and easily to appropriate treatment. That the smoother the tent is, and the more carefully prepared, the less likely will it be to give rise to any cervical irritation, is a self-evident proposition; and of course such tents ought never to be used, except under the most urgent circumstances, when there exists any previous inflammation in the mucous membrane of the cérvix or womb. I am convinced, however, that the tents of laminaria are much less dangerous in this respect than sponge tents, which, however well prepared, must inevitably have rougher surfaces, and which also, from the more rapid decomposition of the fluids they absorb into their cellular structure, are far more adapted to originate and keep up any irritation in a sensitive mucous membrane.

Amongst the accidental complications to which I have already referred as occasional sequelæ of the use of these tents, I need only now mention two, as any others which I have seen have been but of little moment, leaving no bad effects behind. These two accidents, the details of which I think it essential to relate, were a case of pelvic hæmatocele and one of perforation of the cervical wall.

The hæmatocele was produced in this way:—I was hurriedly called some ten miles into the country to see an old patient of my own who was supposed to be dying from hæmorrhage, but no particulars could be ascertained before starting. On arriving, I found that the lady was already so prostrate that it was imperative to stop the bleeding at once. As the hæmorrhage was evidently from the uterus, and probably from a threatened abortion, the patient having passed a couple of menstrual periods, I at once proceeded to dilate the cervix, which was quite unopened, by means of a tangle tent, in order to get at the ovum. The result was satisfactory, so far as saving the patient's life was concerned; but it was only at the expense of a retro-uterine hæmatocele, which appeared

the day after the tent was removed, and confined her long to bed.

The other case—that of laceration of the cervix—was more unfortunate still, as it was evidently the result of a very careless use of the tent. The case occurred in Ward XII.; but as I am relying on my memory for the particulars, I cannot say that I recollect for what purpose the tent was used. Suffice it to say, that it must have been introduced too far within the cervix, as, on examination eight hours afterwards, I found that the os had closed in over the tent, the lower end of which was now buried in the posterior cervical wall, through which it had nearly perforated. After some fruitless efforts at extraction, I was forced to cut open the cervix with long curved scissors before I could effect its removal. Although I have seen other cases in which a similar result has followed the use of tents in inexperienced hands, I certainly never attended any patient who so narrowly escaped a disastrous issue.

So far I have traced, to the best of my recollection, the bad effects which occasionally follow the use of dilating tents made from the laminaria. But I do not wish to imbue any one with the notion that the danger attending their use is so great as to prohibit us from ever having recourse to so effectual a method of diagnosis and treatment. On the contrary, I am so fully aware myself of their value in proper hands, that the accidents which I have related have only impressed me with the necessity of adopting some means to confine such unfortunate events within the narrowest limits; and since I began to employ the precautions I shall immediately detail—precautions which are not given in any of the ordinary English text-books—I have never had any worse result from the use of the tents than the production of slight and transitory forms of cervical catarrh. Those precautions are chiefly:—

1*st*. The non-employment of the tent, so long as we are convinced that there exists any endometritis, or any

recent perimetric inflammation, of whatever nature it may be.*

2d. Tents ought only to be used in the intervals between, never (except under the most urgent circumstances) either during, immediately before, or immediately after a menstrual period.

3d. The greatest care must be taken in the introduction and removal of tents to avoid any violence or force. Any bleeding caused at either time ought not to be interfered with unless it become serious.

4th. The tent should only be used when the patient can remain recumbent from the moment of its introduction to a period after its removal, which varies with each patient. This period ought never to be less than twelve hours, and should usually be much longer.

5th. If the patient complains of pain from the action of the tent, an opiate of some kind—a suppository of morphia often suits—a dose of chloral, or warm water injections, ought to be employed to diminish or relieve it, and thus prevent that restlessness which the pain will inevitably cause, and which is the surest means of producing inflammatory action in the womb or adjoining serous membrane and cellular tissue.

6th. If successive tents require to be employed, it is much safer to allow an interval of twelve hours to elapse between each. A greater interval even would be preferable; but the cervix often closes up rapidly after its dilatation, and if we were to wait much longer, we should frequently have to begin *de novo*.

7th. The tangle tent is fully expanded, and ought to be withdrawn in eight to ten hours after its introduction.

I have thus attempted a slight sketch of some of the accidents following the use of the sea-tangle tents, and of the precautions which such unfortunate results as I have

* To this head we ought to add that it is never very safe to use tents where there has existed any previous perimetric inflammation which has left bad effects behind, either adhesions, for instance, or chronic inflammatory products.

myself witnessed and treated have forced me to adopt I repeat, that since I have used those precautionary measures, I have never had to accuse myself of having been the cause of any accident which might have been reasonably avoided; and it is with the idea that my own experience may be the means of keeping other professional brethren from being stranded on similar shoals, that I have written this paper, and that I have attempted conscientiously, if inadequately, to depict the dangers which may, in most cases, be so easily prevented.

APPENDIX *on the Difficulties occasionally met with in the Introduction and Removal of Tents.*

As the introduction of a tent into the uterine cavity may seem a very simple operation, and one not likely to be more difficult to perform than the use of a sound, I have thought it necessary to add to the foregoing remarks a few observations on some cases which have given me no little trouble in their treatment, and to explain to the best of my ability the difficulties that a youthful practitioner will probably meet with in his first attempts in this direction. That those remarks are not altogether superfluous I can vouch for, as I have seen the most experienced gynækologists more than once completely foiled in attempting to introduce a dilating tent into the cervix.

Whether the tent we intend to use be of sponge or sea-tangle, it is decidedly preferable to commence by expanding the vagina to some extent by a speculum, and for this purpose no better instrument can be found than that of Dr Marion Sims.* In the introduction of the sea-tangle it is by no means so absolutely necessary to use the speculum as in the case of the sponge tents; as the latter, however well prepared, so rapidly imbibe the fluids of the vagina, if allowed to come in contact with them, that they become soft and flexible, and consequently useless, before they can be introduced into the os

* The instrument described in the *Lancet* of 14th May by Dr Meadows might also do for this purpose, though scarcely so well as Sims's.

uteri, and the employment of the ordinary form of Sims's speculum necessarily entails the presence of a third person,* which is occasionally very strongly objected to. The use of any of the varieties of bivalve specula prevents us, of course, from grasping and drawing forward the anterior lip of the os with the tenaculum, and thus frustrates one of the principal purposes for which the speculum is required at all. For the introduction of the tents, too, we require to be provided with different instruments. For the sponge and hollow tangle tents, I generally employ the form of bent stilette, long ago recommended by Sir James Simpson; while the solid laminaria tent is more firmly grasped and more easily manipulated by a long-handled and roughly-serrated modification of the polypus forceps.

Having thus settled the preliminaries, it is by no means difficult, in the great majority of cases, to slip the tent into the os and cervix, but occasionally it happens that you meet with some obstacle to this easy entrance. The most common impediment usually arises from the catching of the point of the tent in one of the folds of the cervical mucous membrane. But this only requires to be mentioned to suggest its own remedy. Much more difficulty, however, is experienced in overcoming the real stricture which often exists at the os internum.† It ought never to be forgotten that the internal or cervico-uterine orifice† is the narrowest point of the canal, and that it is here that the point of the sound is most commonly arrested. In selecting the size of tent to be first used, we ought consequently to be guided by the information which a previous use of the probe has given us of the state of this orifice. In some few cases it will be found impossible to pass a tent, and then the only rational treatment is to dilate the stricture by the pressure of

* One of its advantages, Dr Sims maintains, with some plausibility.

† Of course I am speaking of the normal uterus. I believe that the os externum is the most common seat of congenital or acquired stricture, but a stricture there would probably require division before the tent could be employed at all.

metallic bougies* of gradually-increased diameter. I have seen two such cases in which this was the only feasible treatment. In one of them the strictured internal orifice had apparently been produced by a protracted labour; in the other, it was due to the existence of a fibrous tumour low down in the anterior wall. The treatment would, of course, be similar if the stricture were at any other part of the cervix except the external os. Of spasmodic stricture of the os internum, which is mentioned as a common occurrence, after the use of the sound, by almost all gynækologists, I know nothing from personal experience. We are bound to believe that it occurs, as the weight of authority seems in favour of the statement, but I cannot think that it is so frequent an event as is supposed. Its treatment, if it form an impediment to the introduction of the tent, is obviously to chloroform your patient.

Of all the impediments to the entrance of the tent, however, the most annoying, and probably the most common, is the result of a flexion of the body of the womb on its cervix. The internal orifice, at or near which the flexion almost invariably occurs, is thus most effectually strictured, and the position of the womb is such that it is next to impossible to introduce a straight tent. I ought here to remark, that if the flexion has been produced by adhesions of the uterus to the surrounding parts from bygone inflammatory action, a tent ought never to be used. At least, I know of scarcely any circumstances which would justify the dilatation of the cervix and body of the womb under the imminent risk of a renewal of the peritonitis. To get over the difficulty, I at first thought of employing tents bent to some extent in the same direction as the axis of the flexed uterus, but this was proved by trial to be mnch better in theory than in practice, as at the angle of flexion of the uterus the tent invariably

* Previous incision of the internal orifice could not be recommended if the tent required to be used immediately afterwards. This is by far too dangerous a proceeding to be practised merely for the diagnostic purposes I am here speaking of.

became deeply indented, and thus merely enlarged the cervical and uterine cavities, leaving a strictured part between. I now find that the only real escape from this difficulty is to employ a probe to replace the uterus as nearly as possible in its normal direction, and then to slip in the tent by the side of the metallic director.

In extracting the tent, the only common source of difficulty arises from its occasional unequal dilatation. It sometimes happens that the existence of a stricture of the cervix, whether it be at the internal os or any other part, may produce such an indentation of the tent as to give it an hour-glass form. When this rather awkward result ensues, we ought never to use any great force in our efforts at removing the tent. Grasping the lower end firmly with a three-toothed vulsellum or large artery forceps, and gently moving our hand backwards and forwards with a to-and-fro motion, we can usually succeed without any great traction in bringing out the tent. A much more formidable complication I have already mentioned, as occasionally following the use of a tent which has been pushed too far within the uterus, and over which the os externum has closed. Such a result, however, can only be the sequence either of great carelessness in the use of the tent, or of the employment of too short a one; and, to avoid it, we ought never to introduce a tent under two inches in length. In extracting the tent, Sims recommended us invariably to dilate the vagina by the speculum; but this I seldom, if ever, do, and I never even use any instrument for the extraction, unless the string which is attached to the tent gives way,—a frequent occurrence, however, owing to its often becoming softened by its prolonged soaking in the vaginal discharges. The entrance of air into the dilated cervical and uterine cavities is a danger I certainly never met with, and can scarcely even conceive possible: or even if it did occur, it does not seem likely that it would produce any very injurious result.

CASE OF POISONING OF AN INFANT, SIX WEEKS OLD, BY SIX DROPS OF LAUDANUM—RECOVERY.

By Clarkson Cuthbert, M.D.

July 13, 1870.

On the evening of Saturday, 21st May 1870, I was summoned about 8.30 to see an infant six weeks old, and who was also born at the 7½ months of utero-gestation. I found the patient lying on its mother's knee, quiet and sleeping. It did not appear to be suffering in the least. The parents said it was much pained in the bowels. I caused its clothes to be loosened and examined the abdomen, but could not detect anything wrong. On further questioning, the father told me that during the forenoon and afternoon the child cried very much from pain in the bowels, for which, about 4 P.M., he gave six drops of laudanum; and the reason that induced him to give it was, he remembered a good while ago seeing a certain medical man give ten drops of laudanum to a child three months old, for the same purpose.

On examining the infant more closely, I found it evidently labouring under great stupor; the pupils were contracted to mere points, the pulse thread-like—very rapid and feeble, and the respirations faint, occasionally sobbing-like, and at pretty long intervals; the feet, legs, and body generally were cold and pale. It required a good deal of rough handling to make it cry. The child was fat, well-nourished, and, apparently, before this illness enjoying perfect health.

It was quite clear the infant was labouring under the effects of narcotic poisoning, and I was of opinion, that from the time the infant had swallowed the laudanum, it would be worse than useless to give an emetic. Mustard sinapisms were applied down the spine, and I tried to get a little brandy and water over the throat, but the small quantity that was swallowed brought on such a state of

congestion and asphyxia, that I was obliged to give it up. Seeing now that galvanising gave the only chance, I sat down beside the little patient and applied the poles of an electro-magnetic machine to the chest, sides,—in fact, to wherever I thought it would be best for the purpose of arousing it from its deep slumber. Occasionally mixtures of brandy and milk, and brandy and beef-tea, and plain milk, were injected into the bowel. I may here remark, that whenever the pulse began to fail and the child to show symptoms of sinking, I found the brandy injections of great value, both in raising the pulse and in arousing the infant.

In this way I continued the application of the galvanism for about four hours, when, at that time, I had the satisfaction of getting the patient to swallow a little of its mother's milk from a spoon. The stupor still continued, but the cry became louder and stronger; it was also more easily aroused.

The galvanism was not so vigorously applied now; the patient was allowed to rest for a few minutes, and then it was again applied. About 2.30 A.M. it took the nipple, and again at 4, sucking pretty well upon each occasion.

I saw the patient at 10 A.M., and although somewhat drowsy, it was quite recovered.

REMARKS.—I have been induced to place this case on record, as recoveries from poisoning by opium in young infants are very rare occurrences. Children are remarkably susceptible to the action of opium, and great caution is always necessary in prescribing it for them. Some medical men might hesitate to prescribe so small a quantity as half-a-drop of laudanum for an infant of six weeks, but, in the present case, the father of the patient was rash enough to give a quantity of laudanum equal to nearly half-a-grain of pure opium. It is singular that he should have displayed so much ignorance as to the poisonous properties of the drug, for almost every one knows something about its characteristic effects.

I think no dependence can be placed on his statement

about the medical man giving so much as ten drops to an infant of three months old. No doubt he was mistaken, either in the name of the medicine or in the quantity given.

This case illustrates very well the value of galvanism in poisoning by opium. I have no doubt, had it not been perseveringly applied, along with the brandy and beef-tea injections, the little patient would have ultimately sunk.

I may also state that I have applied galvanism successfully in two previous cases of poisoning by opium, both adult females—patients of Drs Burn and Menzies.

Dr Christison, in his work on Poisons,* relates the following interesting cases illustrating the poisonous action of opium on very young children:—An infant, three days old, got by mistake about the fourth part of a mixture containing ten drops of laudanum. Nothing seems to have been done for its recovery for eleven hours. At that time there was great feebleness and somnolency, but the child could be roused. Artificial respiration was resorted to, but it died in twenty-four hours.

Dr Simson, of this city, communicated to Dr Christison a case of the same kind, where a stout child of fourteen months died in six hours from the effects of three drops of laudanum taken in a chalk-mixture for diarrhœa. In another case, related in the *Lancet* by Dr Kelso, of Lisburn, an infant of nine months died in nine hours after taking four drops of laudanum. And Dr Allison related to Dr Christison a case where an infant of a few weeks old died after receiving four drops of the same drug.

Dr Taylor, in his work on Medical Jurisprudence (page 286), mentions a case of an infant of four months old being nearly killed by one grain of Dover's powder, equal to the tenth part of a grain of opium. Another child of four and a-half years died in seven hours, after having taken four grains of Dover's powder, equal to nearly half a grain of opium.

* Fourth Edition.

A case is also related in the *Lancet* (April 15, 1854), where an infant seven days old died, in eighteen hours, from the effects of *one minim* of tincture of opium, or the *twelfth part of a grain of opium.* Dr Taylor also relates that the smallest fatal case recorded was in the case of an infant, four weeks old, who died with the symptoms of poisoning by opium, in seven hours after a dose of paregoric elixir, equivalent to *one ninetieth* of a grain of opium.

In the same work the author gives cases also where children have manifested an astonishing power of recovery from over doses of preparations of opium. An infant of six months recovered, after having had administered to it ten grains of Dover's powder. Another recovery took place after a child of nine months had swallowed two teaspoonfuls of laudanum. And he relates a third case in which an infant, of between two and three months old, recovered after *five grains* of opium had been given to it by mistake for rhubarb.

CASE OF DORSAL DISPLACEMENT OF THE ARM, A NEW VARIETY, WITH REMARKS ON DORSAL DISPLACEMENT IN GENERAL.

By E. Lambert, Esq., Paris.

July 27, 1870.

Mrs M., 260 Canongate, aged 30; married nine years; at the full term of her sixth pregnancy. A healthy subject, thin rather than stout, of average stature. Four children living. The last two years nine months old. Miscarried twelve months ago; all her labours have been lingering, and all *dry*, except the third.

Labour began at 8 P.M. 2d April 1870.—Waters broke 4 A.M. 3d April.—Sent to Maternity.

Mr Martin was with her at 6 A.M. He found a hand presenting, and made out a presentation of the head and

arm. At 12, he gave a dose of ergot, as the labour did not progress.

At 1 P.M., Mr Image, who had joined Mr Martin, requested my attendance. On examining the abdomen, I found the long axis of the uterine tumour directed from the left iliac region to the right hypochondrium, and by palpation I made out a head lying above the pubes.

I did not suspect anything abnormal. The fœtal heart was heard most distinct at the left lower third of the abdomen. The uterus was evidently firmly contracted around the child, there was neither mobility nor elasticity. Per vaginam, I found, at half a finger's length from the ostium, and lying towards the right foramen ovale, the right hand of the child, with the palm upwards. Following the fore-arm, I reached nothing. The os was fully dilated. I now told the patient that I required to give her chloroform in order to make a sufficient examination; but she positively refused chloroform, and said she would submit to anything that was necessary.

Introducing the hand into the vagina, I found a loop of funis resting on the fore-arm, and following the latter, I reached the left side of the child's head, and assured myself that the right arm was displaced behind the neck; the occiput at the left sacro-iliac synchondrosis. Owing to the displacement of the arm, the head did not lie in the left oblique diameter, but reposed by its left side on the pelvic brim, the displaced fore-arm occupying the oblique diameter. It was clear that version was the only alternative. The child was still vigorous. There was no danger from compression of the prolapsed cord, as the prominence of the chin formed a projecting edge. A messenger was therefore sent immediately to Dr C. Bell, Dr Keiller, and Dr Ziegler, but all were engaged. I again entreated the patient to take chloroform, but she refused.

About 3 P.M., the bladder and rectum having been previously emptied, I introduced the right hand and found the condition just what I have above described. The interior segment of the uterus offered the most ex-

treme resistance to the passage of the hand, and it was fully ten minutes before I surmounted this obstacle. It was even more difficult to make any advance, but fortunately I found the right foot at the epigastrium of the child. I again experienced the greatest difficulty in drawing down the limb, but at last brought it into the vagina, on a level with the hand which had not moved. During this manœuvre, the left hand on the abdomen was endeavouring to lift the head from the pelvis, but failing to move it, I worked the foot between two fingers of the left hand, and pressing the prolapsed arm upwards with the right, requested Mr Image to lift the head through the abdominal parietes by applying both hands. We soon felt the head recede, and the right leg was quickly brought down to the posterior commissure of the vulva. This manœuvre had occupied twenty minutes, and the cord was now pulseless; I worked down the left limb with a finger in the groin, and completed the delivery rapidly. The head caused delay, but finding I could not release the posterior arm readily, I brought down the anterior, and had no further difficulty in releasing the posterior. The child's heart was beating vigorously, but it took a half hour of repeated efforts to bring its respiration to a satisfactory condition. The head was remarkably deformed; the left side of the face swollen from the dependent position, and rendering muscular contractions so obscure, that it produced the appearance of hemiplegia; whilst, on the contrary, the left side of the vault, flattened by long continued pressure on the brim, was to the right what a pent-roof is to a dome. The prolapsed arm of course presented the usual hypostatic congestion. The placenta was expelled within a quarter of an hour. The mother complained of slight pain for some days at the lower part of the abdomen, but was with difficulty kept to her bed for four days.

She is a woman of uncommon pluck. She took a good deal of whisky whilst I was performing version. She is not addicted to the use of spirits, but she declared that

alcohol was as good as chloroform, and described to me a sort of analgia. She said she felt the child's progress without being conscious of pain.

The child recovered perfectly. I displaced both arms the following day, and did not find more difficulty with one than the other.

Dorsal displacement of the arm has only been recognised clinically since Professor Simpson published his work in 1850. He referred on that occasion to two other cases which he had reason to suppose analogous. Foreign journals immediately informed us of three cases in the practice of two living obstetricians. The cases had not been published.

The editor of "Simpson's Obstetric Memoirs" informs us that, in 1855, Dr Simpson had had various communications showing the comparative frequency of the complication.

Mr Jardine Murray published a case in 1861. Dr Eastlake, commenting on this case, states that two of a like nature had occurred at the Rotunda, Dublin, during that year.

Mr W. J. Cox reported another in 1862.

Dr Playfair one in 1867.

Dr Angus Macdonald communicated to this Society two cases in 1868.

Mr Blower, on the publication of Dr Playfair's case, related a displacement of *both* arms in 1845.* This double qualification must atone for poverty of detail. On the occasion of Dr Macdonald's communication, a discussion ensued, when it was elicited that several members had met with like cases. Dr Keiller had observed them. Dr Burn had repeatedly met with these cases, and had no difficulty with forceps. Dr Menzies had met with similar cases, and never had any difficulty with them.

In the second edition of "Simpson's Obstetric Memoirs," we read that Professor Simpson had latterly seen this cause of difficulty overcome without any artificial aid.

* *British Medical Journal*, vol. i., 1867.

Six cases only have as yet been published with sufficient detail to furnish matter for discussion. We may therefore assume that, in their main features, other cases have not differed materially from their prototype. Perhaps Simpson's case in position II. has not been met with since. Dr Playfair's was in III., and my own in IV. We have thus the demonstration that this singular displacement may occur in all the positions of the head, yet is met with rarely except in occipito-anterior.

In order to save time and avoid repetition, I have translated the six cases already published, together with my own—in all, seven—and I proceed to review the whole subject.

Now, from the report of the discussion on Dr A. Macdonald's cases at the meeting of the Obstetrical Society, May 27, 1868, three conclusions may be drawn:—1. That this displacement is not uncommon; 2. That it is not always a cause of dystocia; 3. That such dystocia is readily amenable to treatment. Now, such conclusions from the experience of practitioners who have seen much and well, is of the highest value, but they throw no light on those cases of difficulty which have occurred and will occur. Why is one case simple and another complicated? In the absence of the desiderata furnished by observation, I feel confident that experimentation would throw some useful light on the subject, and I hope that before I conclude I may have interested some one in directing attention to this subject. I will only add that, in our profession, where a question of medical responsibility may at any moment arise, uncertain utterances must be hurtful, and that great interest would attach to an expression of opinion from a body so competent as yourselves.

The first point worthy of note in the study of this abnormal condition is, the frequency of displacement of a single arm. Only one case has been brought forward as a displacement of both arms; we require to know more before our judgment can be influenced by it. All the other known cases—Dr Playfair is silent on the point—

are displacements of the anterior arm. Such a fact is itself remarkable, and I can broach no theory from it.

The theory of causation is difficult enough; the arm of a living child would surely not prolapse, the active movements of a child in utero would with difficulty carry the arm into such an abnormal position, and whether, by passive or active movements, Why should the anterior arm alone be displaced? I am disposed to think that if the arm were partially displaced over the face, uterine contraction might help to carry it over the cephalic spheroid. But why does the posterior arm not suffer displacement? When the case occurred, I had a fœtus at my disposal, and I placed the child through its evolutions on the pelvis. It appeared to me that, in occipito-anterior positions, the inclined plane of the ala of the ilium and in occipito-posterior, the promontory were the causes impeding displacement of the posterior arm, but I do not say that it cannot exist. I also observed that, in occipito-posterior positions with displaced arm, labour was necessarily impracticable, but not always by the same mechanism, for two very interesting varieties of dystocia occur in these latter positions. When the arm is displaced in occipito-anterior positions, if the pelvis is roomy, I believe the elbow may descend by the side of the head. In other cases we know that the arm hitches on to the upper strait in the manner described by Professor Simpson; in the latter case, the occiput being arrested, the head may undergo extension, and a face presentation be produced, as in No. 6.

The arrest of the elbow on the brim may be observed in occipito-posterior positions; or if extension supervenes, a brow presentation may ensue, as in No. 4. But this is not the only disposition observed. Sir James Simpson remarked that no doubt some varieties would be met with in this new form of dystocia described by him, and so it is seen to be. Dr Playfair's case must have been one of arrested fore-arm; but in my case it will be observed that the arm prolapsed, and consequently, the head, pressed

by the contracting uterus, was unable to enter the pelvis. At the same time, the fore-arm, engaged between the left side of the head and the opening os, formed a lever, flexing the head laterally towards the right shoulder, as if it would enter the pelvis in the parietal position; finally, progress was completely arrested when the head itself hitched on the brim, in a position from which, failing version, mutilation alone could move it. Dr Playfair was brought to book with much severity by Dr Eastlake, who rightly aimed his criticism at the diagnosis, though without shaking it. It is strange that he did not inquire which was the displaced arm. His criticism is weak in two points—1*st*. He assumes that displaced arm and hitched elbow are synonymous; and 2*d*. That in all positions of the head, the effect of dorsal displacement would be the same. Experimenting with the fœtus, I saw no reason to question Dr Playfair's diagnosis. I do not so well see how he can argue that when the face presents the obstacle is removed; and we have Case 6 to put against this proposition, for here all endeavours failed until the arm had been reduced.

Dr Eastlake has also criticised the drawing which accompanies Simpson's paper. By a strange oversight, this drawing was given reversed in the Monthly Journal, where the case was first published; and in Dr Eastlake's case, reported by Dr Murray, the same mistake is made. It is not probable that a man of Sir James Simpson's acquirements, and possessing all facilities as he did, would have introduced a rare case with an incorrect figure. Dr Eastlake's case is produced with a more artistic, though adapted copy, I am afraid, of the original, and in the main is more faulty. The sharp tortured lines of the arm in Sir James Simpson's figure are correct. A really true objection lies in the fact that the relations to the pelvis are not shown; and Dr Eastlake would have done good service if he had given us such a drawing of which he felt the need. The impression conveyed by both these figures is that the long axis of the head is carried *to the*

outer side of the oblique diameter, so as to bring into sight the iliac ear. I do not believe that is the true position; and the fact that a face presentation can be effectuated is sufficient proof that the long axis of the head does lie in oblique diameter.

I have been indebted during the last few days to the kindness of Dr Keiller, and to the facilities afforded in his laboratory, for the opportunity of testing my first experimentations, and I hope they may be confirmed by further observation. A drawing was made at the time the case occurred.

We have now to consider the subject of diagnosis.

Abdominal exploration, whether by auscultation or palpation, will not in general give any clue to the abnormal condition. In some cases an elbow may be felt through the parietes, and perhaps its position in relation to the head might excite suspicion. This may be of value in cases such as No. 6, where the head being well advanced in the pelvic cavity, vaginal examination cannot assist us. Again, in the majority of cases this latter mode of exploration is only calculated to mislead; the finger reaches the head, and none of our observations favour the conclusion that we could inspect an abnormal presentation. Simpson only supposes that in two cases where the finger touched an elbow, such a condition may have been present. The ordinary vaginal *toucher*, then, will almost always be insufficient, and can never lead to the full diagnosis, consequently, our practice will be wrong-expectation, a resort to uterine stimulants, or repeated applications of forceps. It is well to insist that the diagnosis cannot be made by the finger; and when we find the head at the brim, whilst our ordinary explorations have not explained the cause of delay, we must assure the diagnosis by the introduction of the hand into the vagina. Once the cause of delay recognised, it will be advisable to proceed at once to remove the complication by reducing the arm. In the case which forms the subject of my paper, the condition was so different that a recourse to

manual exploration was imperative. I found a prolapsed arm, and had to determine whether it was an ordinary head and arm presentation or a shoulder. But on following up the arm I found nothing. It is possible that this itself may be diagnostic. It is well, at all events, to give it prominence for the sake of comparison with future observations. The real interest of the complete diagnosis bears solely on the treatment, and will be considered farther on.

The prognosis in respect to the mother is not serious. We do not know that any woman has died undelivered from this cause; none of our cases have been fatal, neither has recovery been seriously retarded. Two authors mention rupture of the uterus as a complication to be apprehended. Where manual or instrumental interference is required, the result will be contingent on the character of the practice instituted, and on the state of the mother at the time. The result to the child is most unfavourable—two out of seven dead. But we must wait for information as to the frequency of the complication before we venture an opinion. As regards treatment, we have no data by which to determine in what cases expectation may be pursued. Some terminate without intervention; and in a roomy pelvis, doubtless the elbow and head may occupy the cavity together, only rendering labour tedious.

Reduction should be attempted as soon as the complication is recognised. This will probably be accomplished without difficulty, and labour will be terminated by the natural powers, unless uterine inertia has supervened, when forceps or turning may be had recourse to. Where reduction cannot be effected, forceps may yet, in some cases, overcome the obstacle, and terminate the labour, but they should be used cautiously; and wherever version can be performed without risk, it may be resorted to at once. Occipito-posterior cases, with prolapsed arm, require to be turned.

In some rare cases diagnosis is impossible, and the

treatment must be conducted on the general principles which guide us in impracticable labours.

1. Dorsal displacement of the arm occurs more frequently in multiparæ than in primiparæ.

2. Its causes, mode of production, and the frequency of its occurrence, have not been determined.

3. It is a cause of tedious labour, sometimes of difficult, and occasionally of impracticable labours.

4. It occurs in all varieties of vertex presentation, in all the positions of the cephalic extremity, and almost exclusively to the anterior arm.

Of this displacement, two varieties exist:—

1. In the ordinary variety the elbow is on a level with the ear of the same side, and the fore-arm lies behind the neck; the head and elbow may together occupy the cavity of the pelvis, or the fore-arm, resting on the brim prevent descent of the head, or cause it rotate around its transverse axis, producing face or brow presentations.

2. In a second variety, the fore-arm is displaced behind the ear of the opposite side, and the head is placed towards the anterior aspect of the child; the arm occupies the cavity, preventing the head from descending, and producing an impaction which renders further progress impossible.

5. Diagnosis can only be made by a manual exploration of the pelvic cavity.

6. In very rare cases the position of the head renders any diagnosis impossible.

7. Prognosis is favourable to the mother, serious to the child.

8. A displaced arm, arrested at the brim, may be reduced, in general, without difficulty. It has not been demonstrated that this is possible in occipito-posterior positions of the head. A displaced arm prolapsed below the brim cannot be reduced.

9. Both varieties of displacement may be treated successfully by version.

10. Impracticable labours must be treated on general principles.

TABLE representing the General Features of Seven Cases of DORSAL DISPLACEMENT OF THE ARM.

No.	Date.	Reported by	Age of Patient.	Duration of Labour.	Previous Pregnancies.	Position of Occiput and Presentation.	Mode of Delivery.	Event to Child.	General Features of Cases, all Mothers recovered.
			...	Hours.			*Version.*		
1	1850	J. Y. SIMPSON, In Lond. and Edin. Monthly Jl., V. x. 1850, p. 389.		26	9	O. Anterior Right. II. with Left Elbow.	After reduction of Arm to save life of Child.	Alive.	All former labours, easy, rapid. Uterine Inertia.
2	1859	J. J. MURRAY, In Med. T. and Gaz., V. L, 1861, p. 627.	28	?	3	O. Anterior Left. I. with Right Elbow.	*Version.*	Dead.	Last labour very rapid. Cord prolapsed and pulseless. Exhaustion, delirium, &c.' Head was pressed down during each pain, and afterwards receded.
3	1859	W. J. COX. In Dublin Quart. Med., V. xxxiv., 1862, p. 271.	...	?	0	Probably as Case 2.	*Version.*	Dead.	Puerperal Convulsions. Turning very difficult on account of rigid perinæum, and very severe convulsion during extraction.
4	1867	W. S. PLAYFAIR, In British Med. Jl. V. L, 1867, p. 194.	...	?	5	O. Posterior Right. *Brow.*	*Craniotomy.* After forceps twice and lever.	Dead.	All previous labours difficult and tedious. Very large child. Extreme exhaustion. *Diagnosis after delivery.*
5	1864	A. MACDONALD, In Edinburgh Med. Jl. V. L 1869, p. 159.	...	?	3	O. Anterior Left. I. with Right Elbow.	*Forceps.* After repeated failures, reduction of Arm impracticable.	Dead.	Head impacted, not affected by the pains.
6	1868	A. MACDONALD, Idem.	34	12	5	O. Anterior Left. *Face.* with Right Elbow.	*Forceps.* After reduction of Arm, ineffectual before reduction.	Dead.	All her children were large headed. Cord round neck. Head receded in a remarkably elastic manner after each pain.
7	1870	E. LAMBERT.	30	20	5	O. Posterior Left. IV. with Right Arm prolapsed.	*Version.*	Alive.	All previous labours lingering. Cord prolapsed.

TRANSACTIONS

OF THE

OBSTETRICAL SOCIETY OF EDINBURGH,

FOR SESSION XXX.

1870-71.

TRANSACTIONS

OF THE

OBSTETRICAL SOCIETY OF EDINBURGH,

FOR SESSION 1870-71.

NOTICE OF AN ANTEVERSION PESSARY.

By Professor Simpson.

November 9, 1870.

I wish to bring under the notice of the Society an instrument which I have found useful in relieving the distress attendant on anteversion of the uterus, and in doing so, perhaps I may be allowed to make one or two preliminary remarks.

A large proportion of the cases of this form of uterine displacement call for no kind of mechanical treatment. A certain degree of anteversion or anteflexion is to be regarded as the normal condition of the infantile uterus; and after the menstrual function is established, it still persists in a considerable number of women. M. Panas (*Archiv. Gen. de Medicine*, 1869, p. 274) examined 114 women with the view of ascertaining the position of the uterus, and found it straight only in about a third of the cases. His statistics are these:—Uterus straight in 44; anteflexed, 40; anteverted, 12; retroflexed, 3; retroverted, 3; lateriverted, 12. He noted, further, that the uterus was found more habitually erect in proportion with the advance of the patient's age, and that early menstruations occurred most frequently where the uterus was still displaced, the mean age at which the menses appeared in women with straight uterus being 16·3;

whilst the mean age at which they appeared in cases of flexion was 13·2. Beside these congenital cases, we find the uterus becoming displaced forwards as a result of the contraction of peritoneal adhesions, or the growth of a fibroid nodule in its anterior wall, or some morbid influence brought to bear on it during the relaxation of the puerperal period; and even in these the anteversion or anteflexion does not always give rise to such distress as to necessitate our interference.

Again, as is well known, the suffering associated with the displacement is often neuralgic in character, and requires for its relief the use of various nervine tonics and sedatives; or it may be inflammatory, and require the employment of antiphlogistics, etc.

But there are yet other cases, and these not a few, where we cannot afford relief to our patients, except by the application of some mechanical support. I have seen the plan first proposed, I believe, by Dr Moir for the treatment of retroflexion useful in cases of anteflexion. Especially where it is associated with a marked degree of hypertrophy, I have seen the introduction of a sponge-tent straighten the anteflected organ, and set up an absorptive action in the thickened walls. For other cases I have found the intra-uterine stem pessary, with which we are all familiar, yield the most satisfactory results: and in the case which has led to my present communication, no other means that were employed afforded any permanent relief. Various modifications of Hodge's and all other vaginal pessaries were tried for a long series of months and years; the only instrument that seemed to the patient to give any hope of ease being Dr Graily Hewitt's. But even with it the pain in the left groin, which was her most distressing symptom, always remained acute as ever when she turned upon her side, so that she could never sleep except when lying on her back. To sit up or stand was for her an impossibility. The intra-uterine stem pessary was the only instrument that kept the uterus in such a position as to enable her to

obtain the much-desired rest on the side; but, unfortunately for her, the instrument only produced its effect when it was itself kept in position by the wire frame adjusted over the pubes. So great was the relief it gave, however, that, although it produced a degree of menorrhagia, she had worn it continuously for about a year and a half, till, from its pressure on the anterior lip of the cervix, it had begun partially to divide it. When she came under my care I found that by pressing up the fundus with the tips of two fingers the pain was relieved; and in order to keep the fundus permanently supported in this position I got an instrument made such as I now show to the Society. It is a bivalve pessary made of vulcanite, the fenestrated valves having the relation to each other of anterior and posterior, united by a hinge at their lower end, and kept apart by a slight spring. The anterior valve has a notch in its upper free border to receive and support the body of the uterus; the posterior is somewhat longer, and, by pressing upwards the posterior vault of the vagina, supplements the function of the recto-vaginal and the utero-vesical ligaments.

In some cases where I have employed it, the instrument, as I have described it, has proved serviceable; but in the particular case to which I have referred, I found it necessary to adapt a tube to the lower end of it, so as to allow of a wire frame passing from it to be fixed over the pubes. The lady has been wearing it for several months, with the happy result of being able to sleep comfortably on either side. She has, moreover, been enabled to move about the house; and, when at the sea-coast during the summer, she enjoyed open-air exercise in the way of driving and boating to a degree to which for sixteen years she had been a perfect stranger.

Dr Young asked whether the stem pessary was of use in cases of anteflexion, for he had had a case in which it gave no relief.

Professor Simpson had often found the stem pessary of

use in such cases, although in some instances the uterus would still fall forward.

Dr Thomson said he had not had any great experience in anteversion or retroversion, but was of opinion that an instrument such as Professor Simpson had described must be of great benefit in many cases.

Dr Keiller said that he had repeatedly shown to the Society pessaries made of gutta percha and vulcanized india-rubber. One of these consisted of a couple of ring pessaries, which, when put together, made an instrument somewhat similar to that described by Professor Simpson. At a previous meeting, he mentioned that two of Hodge's pessaries, united by the one being pressed into the ring of the other, fixed themselves. He thought it an advantage to have the rings separate. One great advantage of having the instruments made of gutta percha is, that they can be moulded, and applied not laterally, but behind and in front. It is difficult to cure retroversion, which occurs most frequently in women who have had children, whereas anteversion is most common in virgins. It often occurs, however, in married women, and is a cause of sterility. He was of opinion that anteflexion was more easily cured by Professor Simpson's instrument than anteversion. The great difficulty is to keep the instrument in position, and yet not prevent intercourse. He had operated according to the plan recommended by Marion Sims, and in one case the patient afterwards became pregnant. He thought Professor Simpson's pessary was likely to be very useful, as there was no intra-uterine stem. Dr Keiller's experience did not quite bear out the favourable opinion entertained by Sir James Simpson as to the intra-uterine stem pessary. He thought it was of great importance to have the anteversion portion of the pessary spread out, so as to stretch the anterior wall of the vagina.

Professor Simpson made some remarks in reply to the observations that had been made.

ON A CASE OF PUERPERAL MANIA TREATED BY CHLORAL.

By R. C. Furley, Esq., L.R.C.S.E.

November 9, 1870.

There are three periods during which a female may be attacked by puerperal mania—these are during gestation, delivery, or after parturition. Mania during the first of these periods, I believe, is rare, although I have seen one case which seemed to have been caused by tight lacing, employed for the purpose of concealing the pregnant condition, and probably also occasioned more or less by the mental distress which concealment in illegitimate cases entails. The case was singular in its character, inasmuch as it continued during a week of the period of gestation, during delivery, and long after parturition had taken place. Attacks of mania during the second period —viz., that of delivery—are very common, indeed the most common of the three; and although temporary in their character, and not very important in their nature, still, as they frequently come under our observation, they deserve a passing remark, if it were only to point out that the late Dr Montgomery was the first to describe the delirium that occurs during labour; and his description is so faithful, that I cannot refrain from quoting his words. Dr Montgomery observes, that mania at this period "comes on suddenly during perfectly natural labour, and most frequently during the dilatation of the os uteri. It is not accompanied nor followed by any other unpleasant or suspicious symptom; it occurs, perhaps, immediately after the patient has been talking cheerfully, and having lasted a few minutes, disappears, leaving her perfectly clear and collected, and returns no more, even though the subsequent part of the labour should be slower and more painful. In every instance which came under my observation, the patients were conscious that they had

been wandering, and occasionally apologised for anything wrong they might have said, although they were not aware of what the exact nature of their observations might have been." * The above description corresponds almost exactly with what all of us have again and again seen. Dr Montgomery attributes this momentary incoherence, and I think correctly, to the forcible distention and dilatation of the cervix,

But by far the most important period at which puerperal mania occurs is after parturition, because then the mania which supervenes is of a much more severe and permanent character—requires careful supervision and cautious treatment. Up to the present time, any plan of treatment, even though recommended by the most eminent authority, has been found very faulty; and the practitioner who has had the misfortune of having a case of puerperal mania to attend to, has been obliged invariably to abandon the treatment laid down in books and expressed in lectures, and trust in a great measure to his own resources, or to rely mainly upon those principles inculcated during his student career, and intended to guide him during his professional life. As I am unable to contribute any precise information as to the causes of puerperal mania during gestation, and as I think Dr Montgomery has explained the reason of it occurring during delivery, I hope I may, without seeming to be presumptuous to my seniors in practice and ability, make some remarks as to the cause of this disease after parturition. The excitement of puberty—the susceptibility of the menstrual periods—the sympathy of the mammæ with the uterus, when the latter is affected,—all point to the fact that the generative functions of the female are productive of a highly impressible condition of the nervous system at any of these changes. Even the time of life when the procreative function ceases, is not exempt from excitability. Now, if there be unusual excitability at puberty, during the menstrual period, at delivery, after parturition, and during

* *Dublin Journal*, old series.

lactation, is it not reasonable to suppose—nay, can you doubt the fact—that there will be a greater amount of excitability if any of these processes or functions are deranged or entirely suppressed? The suppression of the mammary secretion and that of the uterus, the lochia, the excitability caused by either or both of these, added to the already excited condition of the nervous system, will, I think, fully account for the production of puerperal mania after parturition, and more especially in an individual of a naturally nervous temperament.

I will not touch upon the statistics of the disease, because any that I have been able to obtain are not of a very reliable character, and are chiefly got from asylums for the insane; and those patients who are sent thither form, I am persuaded, but a small proportion of those which actually occur, as every medical man hopes—for he cannot predicate—that his patient will recover within a few weeks, and he endeavours to avoid sending her to an asylum out of compassion for the feelings of relations.

It is unnecessary to make any remark upon the symptoms of the disease, because these, although differing in detail in each individual case, present prominent features of resemblance in all.

It is to the treatment pursued in the following case that I wish more particularly to direct your attention, because, although not the first, it is among the first cases of this disease that have been treated by that new and remarkable substance chloral. So far as I am aware, Dr Adams of Lanark was the first in this country to record a case of puerperal mania treated by it; but as there are points of difference in the manner of the exhibition of the drug in his case and mine, I think it will be of advantage to show the distinction, as the result seems much more favourable to my mode than his. Dr Adams, in his communication to the *Lancet* of 22d January 1870, states that he gave the chloral on twelve separate occasions, and once at an interval of a fortnight. His method then appears to have been to administer the remedy only at

the time of extreme delirium, whilst I, as will be subsequently gathered, administered it continuously. I have, of course, no means of judging of the comparative severity of the two cases, because language almost fails to convey the appearance, expression, and incoherence of a person whose mind is wandering. But the details of the case will, I trust, furnish you with some idea, even though a faint one, of its severity. I must premise its narration, however, by saying that I was not the accoucheur, and my knowledge of the patient's condition previous to being called in to see her is derived from the midwife who was in attendance.

Mrs W., æt. 21, a primipara, sent for the midwife engaged to attend her at 5 A.M. on the 7th of June. She had been complaining during the night of slight pains, but not sufficiently strong to warrant her in disturbing any one. Shortly before five o'clock, however, the pains became much more severe in their character, and she deemed it necessary to send for her attendant. When the midwife arrived, she found the os well dilated, and the head descending in the first position. The labour progressed quite naturally, and she was delivered at 8 A.M. The convalescence proceeded satisfactorily until the eighth day, when the secretion of milk and the lochia were very much diminished; she continued in this state for three days, when I was called in, and found her in the following condition.

The patient was sitting up in bed with the infant on her lap, tossing it from side to side, and rolling it about. When the baby was taken from her, she rose up and made for the window, and said she wanted to go out by it; her long hair was dishevelled, and was hanging partly over her face; her countenance was pale, and had an appearance calculated to arouse suspicions of albuminuria. There was no urine to be obtained for examination, and I had to trust to the look referred to. I was informed she had not slept for three days and nights. This, my first visit, was on the evening of the 18th of June. I gave the usual

directions as to her being carefully watched, and ordered a dose of castor oil immediately, and half a drachm each of chloral and acetate of potass every four hours. My reason for giving the chloral, of course, is sufficiently obvious,—viz., as a sedative to the nervous system, and with a view to procure sleep. The acetate of potass was administered to diminish the supposed condition of albuminuria, which her appearance had strongly made me think was present; and my former experience of the acetate, especially in puerperal convulsions, gives me thorough reliance on its efficacy. On the morning of the 19th, when I called, I ascertained that, twenty minutes after she got the first dose of chloral, she fell asleep and slept for three hours. She still talked nonsense, wept occasionally, but was much calmer than on the previous evening, and had taken tea, toast, and an egg for breakfast. The examination of the urine, at least by heat, did not show, as I had suspected, the existence of albumen. I ordered the chloral and potass, however, to be continued at the same intervals, and in the same quantity. The next day, the 20th, she was much quieter—had occasionally talked rationally, and had taken her food willingly. Ordered a pint of beef-tea, and medicine to be continued.

On the 21st, I found she had slept from 10 P.M. till 5 A.M., was much more rational, and had taken her food well. She occasionally tore her hair, and it being impossible to keep it in anything like order, it was removed, with the exception of a small portion in front, preserved for the sake of appearance. The same directions as to food and medicine were given. On the 22d I found that she had slept the whole night, talked rationally, answered questions coherently, was a little peevish, and wondered what had been wrong with her. On the 23d, she seemed quite rational, but very weak. Ordered medicine to be given every eight hours, instead of four hours as before. On the 24th, six days after I saw her first, and nine from the invasion of the disease, she was able to be up for an hour, and was quite rational. On the 25th, 26th, and 27th, she

was up the greater part of the day; and on the 28th, ten days after my first visit, when I called I found her dressing the baby, and attending to her household duties. I looked in upon her for some days subsequently. Her milk gradually returned, and she was able to suckle her child.

I am thoroughly convinced that the chloral in this case was the agent which produced so beneficial a result, and I feel perfectly satisfied, from my experience of it in this and other diseases, which I may at some future time bring under your notice, that what chloroform is to surgery, chloral will be to medicine.

Dr Thomson alluded to the case he had reported to the Society in which chloral was very useful as a calmative. The patient, however, was now in Morningside. He thought there might have been some kidney affection in Mr Furley's case, which perhaps was benefited by the acetate of potass.

Dr Keiller mentioned a case, which he had alluded to before, in which great success was attained by feeding the patient under chloroform by the stomach-pump. She soon got quite well.

Dr Frazer said that acetate of potass was changed into the carbonate in the system; and, as chloroform was evolved from chloral when it met with an alkali, this might account for the success of chloral in Mr Furley's case. The acetate also keeps up the quantity of urine, which chloral tended to diminish.

Dr Cairns was inclined to think that, had any other sedative been used, it would have done as well as chloral.

Dr Gordon thought that small doses of chloral were safest and best.

Mr Furley said, in reply, that his reason for giving the acetate was, because the appearance of the patient led him to suspect the presence of albumen.

ON THE IMMEDIATE TREATMENT OF LACERATION OF THE PERINÆUM.

By Thomas More Madden, M.D., M.R.I.A., Dublin.

(*Communicated by Dr Ritchie.*)

November 23, 1870.

As some difference of opinion still exists as to the propriety of immediate curative treatment in cases of laceration of the perinæum occurring during childbirth, I venture to hope that the following observations on this subject may not be altogether devoid of some practical interest. I may premise, that I have had for some years a rather extensive opportunity of seeing such cases not only in the wards of the largest lying-in hospital in the United Kingdom, where, however, from the care and caution constantly observed, this accident is comparatively rare, but also, and more frequently, in connexion with the Extern Department attached to the hospital, as patients oftentimes send up to the hospital for assistance only after the occurrence of the accident—having been attended during their labour by some neighbour or equally ignorant midwife, and thus the comparatively large number of cases of laceration of the perinæum which had fallen under my observation may to a great extent be accounted for.

Laceration of the perinæum is either a very trivial or a very serious accident, according to its extent. There are three principal varieties of laceration which require consideration. 1*st.* That in which merely the fourchette and anterior fibres of the perinæum are torn; and this may be regarded as a very minor accident, as it commonly occurs in first labours, and generally heals of itself in a few days. 2*dly.* Laceration may extend through the entire extent of the perinæum from the fourchette to the sphincter ani, the latter remaining intact. This is always a serious complication, and was the most frequent variety of the

accident which came under my treatment. *3dly.* Laceration of the perinæum may extend through the sphincter ani, and in some, fortunately comparatively rare, cases may tear through the recto-vaginal walls. This is, with one exception, the most grievous accident of a non-fatal nature which can happen to a woman in childbed.

Even where neither the sphincter ani nor rectum is torn through, the consequences of laceration of the perinæum are very painful. This accident generally occurs in cases where the perinæum is not properly supported during labour, and hence naturally is most frequently met in patients in the humbler class, who have not availed themselves of the benefits of some lying-in institution, but who have been attended by an ignorant midwife. Now, the consequences of laceration of the perinæum are even more distressing in patients of the working class than they are in those of a better condition; for the wives of our artizans and labouring men endure more hardships and go through more labour than their husbands. The whole of their domestic duties—washing, cooking, carrying children, etc.—all act as powerful predisposing causes of prolapse of the womb; and how much the more must these women be liable to this, when the support afforded by the perinæum being destroyed, not only is the tendency of prolapse increased, but also the chance of restoring the parts to their natural position, and of keeping the uterus from prolapsing by a pessary, is rendered hopeless.

The causes of rupture of the perinæum may be divided into two classes. The *first* embracing all those which predispose to this accident, such as parturition for the first time at a very advanced or very early age, when the parts are rigid and undilatable. *2dly.* Rigidity of the perinæum from any other cause. *3dly.* A peculiarly long perinæum, or, in some rarer cases, a remarkably short one, the parts being so small that the fourchette closely approximates to the margin of the sphincter, and in which the vulva is so small that there is, as it were, no room for the transit of

the child *per vias naturales*, and nature has to make an artificial passage for this purpose. And, *4thly*. A thick, puffy œdematous state of the perinæum constituted the predisposing cause of laceration in some of the cases referred to in this paper. The exciting causes of laceration of the perinæum are, 1*st*, and most commonly, the neglect of the accoucher in not affording proper support to the perinæum during the birth of the child's head and shoulders; 2*dly*, and generally as a consequence of this, the sudden expulsion of the head or shoulders before the parts are sufficiently expanded to admit of their passage with safety; and, 3*dly*, the ignorant application of the forceps, the accoucher neglecting to remove the posterior blade sufficiently early, or making pressure through the expanded perinæum on the instrument during the delivery of the child's head.

The first modern surgeon to propose the immediate treatment of lacerations of the perinæum appears to have been Dieffenbach of Berlin, whose operation, as performed by him so far back as 1829 (although many modifications have been introduced since then by Mr Baker Brown and other recent authorities), is substantially the same as that which we have found so successful in the practice of the Dublin Lying-in Hospital.

The operation proposed by Mr Baker Brown, for what I have described as the second variety of laceration of the perinæum—namely, rupture extending up to but not through the sphincter ani—is a very elaborate one, including the division of the sphincter on both sides to the extent of an inch, or even two, external to the anal orifice, by an incision carried outwards and backwards about a quarter of an inch in front of its attachments to the os coccygis, and the insertion of both quilled and interrupted sutures.* This operation is essentially different from the comparatively simple procedure by which the same condition has been treated with some success in the Rotundo, as will be seen by the cases

* Diseases of Women admitting of Surgical Treatment, p. 36.

detailed in this communication. By immediate treatment, I mean the introduction of the sutures within two hours from the occurrence of the accident. In the great majority of cases, however, the sutures were introduced as soon as the placenta was expelled.

The kind of suture adopted in all the cases of lacerated perinæum on which I have operated has invariably been the interrupted; and though most authorities recommend either the quilled or twisted suture, I have found that employed in the cases referred to in this paper the most readily used and the most satisfactory in its result.

Sutures of various materials were tried in some of the cases on which I operated, as, for instance, silk and hemp, steel and iron wire, catgut, and Lister's carbolized sutures. But after a trial of all these, we came back to that with which we had first commenced—namely, tolerably fine silver wire. The silver wire appeared to produce much less irritation and danger of sloughing out than any other, and I believe, more quickly brings about the coaptation of the lacerated surfaces which are held in contact by it.

It is essential to remove the sutures as soon as union has been effected, and before any suppuration has taken place; and to the observance of this rule it is that I mainly ascribe the successful termination of so many of the cases now referred to. In the great majority of cases in which the immediate treatment of laceration of the perinæum is not successful, I am fully convinced by experience that this is due to the fact that sutures were left in till they cut their own way out by suppuration. As a rule, I have found from thirty-six to forty-eight or fifty hours sufficiently long to have the stitches in in these cases; and I can hardly understand the success of cases narrated by some writers, who recommend the sutures to be left in for six or seven days.

The sutures must, in all cases, be aided by great attention to cleanliness, so as to prevent the edges of the lacerated surfaces from being irritated by the lochia and

other discharges. For the same reason, for some days after the operation, the patient should not be allowed to pass water herself, but this should be drawn off twice or three times daily by the catheter. The bowels must also be kept confined till permanent union has been effected; and in general this can be managed by avoiding the administration of the usual laxatives until a couple of days at least after the removal of the stitches, and by attention to the patient's diet. Until perfect union has been obtained, or till all hope of it has been abandoned, no solid animal food should be given, but the patient's strength must be well supported by wine, strong broths, and farinaceous diet. It is obviously essential that the parts should be kept in the closest apposition, and the patient must be prevented from moving by a tight binder round the hips, with pads on either side of the nates, so as to give support to the sutures. In some cases I have covered the perinæum with a layer of collodion for the same purpose, but I have not found this to answer, as the collodion generally caused such intolerable irritation to the raw surfaces, that I was obliged to remove it; and, besides, it prevents the discharge from escaping. The knees must be also bound together with a pad interposed to prevent their stripping from the pressure. After the operation, a full opiate was generally administered, not only to prevent any action of the bowels, but still more to allay the local pain and soreness, and to diminish the constitutional irritation which might otherwise follow the introduction of the sutures.

The following table is illustrative of the foregoing observations. In it I have confined myself to those cases of laceration of the perinæum which have come under my observation within the past six months. These amount in all to twenty-one; in four of these the operation failed; in two it was partially successful; and in fifteen cases it succeeded completely. Seventeen of the patients were primipara, two cases were second pregnancies, one was a fourth labour, and another was the patient's eighth de-

livery. In ten laceration was caused by the child's head, in nine by the shoulders, and in two by both head and shoulders. In eleven instances the patient was delivered of a female child, and in ten of a male child. Two were forceps cases, one was a breach presentation, and in all the other cases the labours were natural.

No.	Name.	Age.	Pregnancy.	Cause of Laceration.	Sex of Child.	Lbs. Weight.	Sutures used.	When Introduced.	Removed.	Result.
1	C. S.	21	1	Head and shoulders.	Female.	8½	3 wire & 1 silk.	April 18, noon.	Ap. 21, 10 a.m.	Successful
2	A. M.	19	1	Head.	Male.	6¾	2 iron wire.	„ 22, 6 a.m.	„ 24, 11 a.m.	Failed.
3	C.M'C.	22	1	Head.	Male.	7½	4 silver wire.	May 6, 11½ p.m.	May 9, 10 a.m.	Successful.
4	T. B.	28	4	Shoulders.	Male.	6½	3 silver wire.	June 1, 12½ a.m.	June 3, 11 a.m.	Successful.
5	C. T.	19	1	Head and shoulders.	Female.	8¾	3 iron wire.	„ 9, 6½ a.m.	„ 11, 1 p.m.	Partially succeeded.
6	T.F.P.	29	2	Shoulders.	Male.	7	2 silver wire.	„ 29, 3½ a.m.	July 1, 11 a.m.	Failed.
7	A. L.	23	2	Head.	Female.	8	3 linen thread.	July 3, 10 a.m.	„ 5, 11 a.m.	Successful.
8	J. F.P.	18	1	Head.	Male.	7¾	2 silk sutures.	„ 6, 7 a.m.	„ 10, 11 a.m.	Failed.
9	A. M.	23	1	Head.	Male.	7	1 silk suture.	„ 8, 6½ a.m.	„ 10, 11 a.m.	Successful.
10	A. C.	23	1	Shoulders.	Female.	6	2 silver wire.	„ 10, 2 a.m.	„ 12, 9 a.m.	Successful.
11	E. H.	26	1	Head.	Male.	8	2 iron wire.	Aug. 9, 9 p.m.	Aug. 12, 9½ a.m.	Successful.
12	J. W.	27	1	Shoulders.	Female.	8	2 silver wire.	„ 22, 2½ p.m.	„ 24, 9 a.m.	Successful.
13	K. M.	27	1	Head.	Female, stillborn	...	2 silver wire.	„ 24, 4 p.m.	„ 26, 1 p.m.	Successful.
14	T. L.	19	1	Shoulders.	Female.	6½	2 catgut.	Sept. 16, 1 a.m.	Sep. 18, 1 p.m.	Successful.
15	M.A.C.	22	1	Head.	Male.	6½	2 silver wire.	„ 20, 4 p.m.	„ 22, 9 a.m.	Successful.
16	A. N.	40	8	Head.	Female.	7½	3 iron wire.	„ 28, 1 a.m.	„ 30, 10 a.m.	Successful.
17	B. K.	23	1	Shoulders.	Male.	8½	2 silver wire.	Oct. 6, 2 a.m.	Oct. 8, 11 a.m.	Successful.
18	T. T.	19	1	Shoulders.	Female.	7½	2 silver wire.	„ 7, 3 a.m.	„ 9, 11 a.m.	Partial success.
19	J. M.	25	1	Shoulders.	Female.	6¾	2 silver wire.	„ 8, 7 a.m.	„ 10, 9 a.m.	Successful.
20	S. P.	34	1	Head.	Female.	6¾	3 wire sutures.	„ 22, 6 p.m.	„ 24, 10 a.m.	Failed.
21	M. C.	24	1	Shoulders.	Male.	6½	2 iron wire.	„ 28, 9 p.m.	„ 30, 11 a.m.	Successful.

1. Forceps case; laceration caused by patient's resisting the support of perinæum during the extraction of head and shoulders.
2. The patient suffering from primary syphilitic ulcers.
3. Laceration through sphincter ani.
4. Extensive laceration from shoulders through sphincter; sphincter united. Purgatives given by nurse's mistake.
7. Laceration through sphincter ani.
8. Sutures introduced too late, and sloughed out.
9. Very extensive laceration.
10. Difficult, second order; forceps.
12. Preternatural; breech.

In addition to the above, the following cases may also be recorded:—

15*th July* 1869.—S. B., aged 28, sixth pregnancy, was delivered of a healthy male child weighing 7½ lbs., after a natural labour of five and a half hours, being only forty-five minutes in the second stage, and thirty minutes in the third stage. There was extensive laceration of the perinæum, extending up to, but not involving, the sphincter ani, caused by the sudden expulsion of the head, the perinæum not being properly supported. I was immedi-

ately sent for, and introduced two silver sutures, which were removed in forty-eight hours, when perfect union was found to have taken place; and she was discharged home on the tenth day after the accident.

18th July.—E. C. was delivered of a male child, weighing 5 lbs. 4 oz., after a natural labour. It was her second pregnancy, and there was extensive laceration of the perinæum, occasioned by the passage of the shoulders through the vulva. Two sutures were introduced, and removed in forty-eight hours. The anterior one failed, the posterior succeeded, and saved one inch of the perinæum.

26th June.—Theresa Fitzpatrick was delivered by the short forceps of her first child, a male, weighing 7 lbs., at 3½ A.M. There was laceration of the perinæum about one inch in extent. I introduced two silver wire sutures immediately after the expulsion of the placenta. The wires were removed 1st July, at 11 A.M., but no union had taken place; owing, I believe, chiefly to the fact that the nurse had administered purgative medicine by mistake the preceding day.

6th August.—E. L., aged 30, first pregnancy, was delivered by myself by the short forceps after a labour of thirty-eight hours, the head having been delayed on the perinæum for nearly five hours from inertia. The child was a female, weighing 7 lbs. 2 oz. There was considerable laceration occasioned by the shoulders. Two silver sutures were introduced, and removed on the 9th with success.

20th August.—E. B. was delivered of her first child at 11 A.M. She was 26 years of age, and had been six hours in the second stage when I applied the forceps under chloroform. The delivery was easy, but the perinæum was extensively lacerated at the head; the laceration did not extend to the sphincter. Two silver wire sutures were introduced, and removed on the 25th at 10 A.M., when the anterior was found to have caused sloughing, but the posterior suture succeeded, and the patient was discharged on the 29th inst., with a tolerably fair perinæum.

12*th September*.—A. B. was delivered of her first child, a male, weighing 8½ lbs., after a natural labour of sixteen hours. The passage of the shoulders occasioned extensive laceration of the perinæum, the sphincter ani being completely torn through. I at once brought the parts together by three silver wire sutures, which were removed on the 14th with perfect success.

30*th September*.—M. E., delivered of her first child, labour natural; child male, weight 7 lbs. 5 oz.; extensive laceration from head. I did not see her till half an hour after expulsion, when I introduced two silk sutures. The patient was very turbulent, and refused to inhale chloroform. I removed the stitches in sixty hours. The perinæum was completely united.

21*st October*.—M. S., aged 27, delivered of second child, a male, weighing 9 lbs., after a natural labour of ten hours. The perinæum was very long and rigid, and the child's head being very large, caused laceration, extending through the sphincter into the rectum. I saw her within a few minutes, and introduced three silk sutures, which failed to produce any union.

24*th November*.—M. C., aged 22, delivered of first child, a male, weighing 8½ lbs., after a labour of thirty hours, rendered difficult by the great size of the child's head, and necessitating the use of the forceps. The instrument, however, was removed before the head passed through the vulva, as is usual here in such cases. But, notwithstanding this precaution, the immense size of the head and preternatural smallness of the parts, caused a laceration extending through the sphincter. Silver wire sutures were at once introduced, and, when removed on the third day after the operation, the parts were found completely united.

6*th May* 1870.—Eliza M'Carthy, aged 22, first pregnancy, was delivered of a male child, weighing 7¼ lbs. She was attended by a French gentleman not accustomed to English midwifery practice, and who did not support the perinæum. The result was, the passage of the head

occasioned very extensive laceration of the perinæum, extending through the sphincter ani, at least the external sphincter. I was sent for before the expulsion of the placenta, and immediately afterwards introduced four silver wire sutures. The legs were bound together, a binder applied round the hips, and a full opiate administered. The stitches were removed on the 9th, when firm union was found to have taken place, and she recovered perfectly.

1*st June.*—Theresa Beatty, confined at 4 A.M. of fourth child, a male, weighing 9½ lbs.; the labour was natural. There was extensive laceration of the perinæum, the anterior fibres of the sphincter ani being torn by the child's shoulders. I inserted three silver wire sutures. The two anterior sutures were removed the following night at 11 P.M., and the posterior one twelve hours later. The result was perfect union, and she went home convalescent on the 8th.

9*th June.*—Catherine Toole, aged 19, first pregnancy, was delivered of a female child, weighing 8¾ lbs., 6.30 P.M. Labour natural. The patient was so excited and unruly during the birth of the child that the perinæum could not be properly supported. The child was very large, and there was laceration of the perinæum, extending through the anterior fibres of the sphincter ani, occasioned in part by the head, and still more so by the shoulders. Three fine iron wire sutures were at once introduced (I first put the patient under chloroform). Her legs were fastened together, an opiate administered, and the catheter was passed twice a day. The wires were removed on the 11th at 1 P.M., when it was found that the anterior sutures had failed, but that through the sphincter ani had succeeded.

Dr Cochrane thought the paper was a most practical one, and concurred with most of the author's opinions.

Dr Pattison did not think that there was anything new in Dr Madden's paper. The operation described was just

what had been practised here for a very long time. He was of opinion that Dr Madden was wrong in removing the sutures so soon.

Dr Keiller considered Dr Madden's communication a very useful, because a very practical one; although he so far concurred with Dr Pattison in the opinion that the paper contained little that we did not know, or had not often spoken of here before. The operation described was the one he had himself long practised, if there was any serious laceration. He preferred to operate at once rather than wait for any length of time. In cases of laceration the patients were not usually aware that any mischief had occurred, and it therefore rested with the attendant to recognise its extent, and, if severe, to take immediate steps to remedy the mischief. He had seen a case very lately, with Dr Dickson, in which the perinæum was extensively ruptured, and the stitches were introduced at once. The patient did well. When the fourchette is ruptured, it does not heal so readily as supposed. In cases of rupture, he was in the habit of putting in two deep sutures posteriorly, and none anteriorly. He was of opinion that the anatomy of the female perinæum was too little studied by students. In operating on the perinæum, the greatest care should be taken afterwards to keep the parts in apposition, and this is often best accomplished by drawing the hips together with plaster. As a rule, he kept the stitches in three or four days. In regard to the more serious operation of Baker Brown, which he had very often performed, it was a mistake to suppose that it was necessary to cut through the sphincter ani, especially if quill sutures were used. He had used carbolized ligatures with success. Formerly, he was in the habit of allowing weeks to pass before operating, but now usually adopted immediate operative treatment, as indicated in the paper now read.

Professor Simpson remarked that it would have been of importance to know at what date after confinement Dr Madden's operations had been performed. Many cases of laceration heal themselves without any surgical interference. He was of the same opinion as the gentlemen who had spoken previously, that the stitches were taken out too soon, for, unless there is very much constriction, little irritation or ulceration is ever caused. In deep sutures he did not think that it mattered much whether

the ligature used was carbolized or not, as they do not need to be left in long.

Dr Sidey thought that Dr Madden's success was very poor, and that Nature unaided might do as well. He had a case on Sunday of rapid labour, in which the perinæum was torn, and to-day the tear is healed, although nothing was done. He recommended the use of horse-hair ligatures in such cases, as they cause no irritation.

Dr Keiller asked if, in the case now mentioned, care was taken to examine the extent and depth of the supposed union. It often happened that the union was quite superficial, leaving a very thin, and subsequently useless, perinæum.

Dr Bruce corroborated what Dr Sidey said in regard to the horse-hair ligatures. He thought that it was unnecessary to operate earlier than two or three days after confinement.

Dr Wilson remarked that the great majority of cases of rupture healed well enough without any interference.

Mr Furley thought that Dr Madden had omitted to say how the wound was to be kept clean.

Dr Rattray had seen two or three cases where the rent healed beautifully with the carbolic acid lotion.

ON THE INFLUENCE OF CHILDBEARING ON THE MUSCULAR DEVELOPMENT OF WOMEN.

By Alfred Wiltshire, M.D., M.R.C.P.L., London.

(*Communicated by Dr James Young.*)

November 23, 1870.

Perhaps some who read the above title will wonder what childbearing can have to do with the development of maternal muscles, other than those pertaining to the uterus, the increase of which during pregnancy is too well known to call for special comment in this connection. The writer trusts, in the following brief remarks, he may be enabled to show that observation and reflection upon some of the conditions of childbearing are suggestive of the influence of that process in the manner indicated.

That muscles are increased by use, provided always that the use be not excessive, is well known. The well-worn illustration of the blacksmith's arm will be a sufficient example of this physiological law.

It will not be difficult to show that the conditions of childbearing are favourable to muscular development, and that in one stage or other of the process the muscles of nearly the whole of the body are engaged.

Before proceeding further, however, it will be well to explain that the term *childbearing* is used to signify something more than mere gravidity; it comprehends not merely the pregnant state, but also the process of lactation, or period of suckling, for certain muscles are specially influenced by each.

During pregnancy the weight to be carried (the gravid uterus, liquor amnii, placenta, and fœtus,) demands, as it increases, augmented power on the part of the muscles concerned in carrying it; and that the latter respond to the call made upon them is clearly a fair, and indeed unavoidable, inference. Now, the muscles chiefly concerned in the pregnant state are those of the back, loins, hips, legs, and abdomen. Those of the back are more and more called into play as the burthen increases; and the attitude of a pregnant woman when walking or standing betrays the reason of this; for we see that she endeavours to keep the centre of gravity in the required line by throwing the lumbar region forwards, and the upper dorsal backwards, in an increasing degree as pregnancy advances. The thick muscles of the back, then, must be engaged in this effort, and consequently they become more developed than in the non-pregnant state. No explanation is required to show that the muscles of the lower part of the body are engaged in the pregnant state, for it is obvious that, as the lower limbs have to sustain and carry the weight of the body, they must be correspondingly developed.

Nor need one labour to prove that the abdominal muscles must also share in the general increment, for that

will be equally obvious. It may, however, be well to remind the reader that the attachments of the abdominal muscles are such as to elicit increased action on the part of muscles not, so to speak, directly concerned: thus the upper attachments of the recti necessitate some action on the part of the muscles which fix the thorax; and a reference to the other muscles of the abdominal parietes will show the same thing. Then, too, the psoas and iliacus muscles and their neighbours partake in the general increment of the muscles of the lower half of the body.

Thus, then, briefly stated, pregnancy calls into greater activity all the muscles of the lower half of the body, or, roughly speaking, all that lie below the mammary zone. This endowment of the muscles of the trunk and limbs with greater power during pregnancy, with which it (the power) increases *pari passu*, appears to me to have practical bearings; it is but another illustration of the wondrous harmony and economy of Nature, for this same muscular development which is called forth by the pregnant state is of infinite service at that supreme moment when the offspring is brought forth. These muscles which, up to the moment of delivery, have been gradually gaining strength, now, when the final act of an important period has come to pass, are of untold value, for by their aid the womb is enabled to cast off its burthen with the least amount of risk, suffering, and effort. Thus does Nature, with inimitable thrift, produce large results with small means.*

Then, when parturition is over, suckling brings another set of muscles into action, those of the upper half of the body. Now, to suckle a child, a mother must take her babe into her arms, and generally she uses both arms; thus, the act of suckling brings into play all the muscles of the upper limbs, dorsal as well as pectoral. And as this is an act which is being almost constantly performed for several months together, it follows that there must be

* The Rev. Dr. Haughton has admirably shown the important part played by the abdominal muscles in the parturient act.

some increase in the development of the muscles employed. The act of carrying a child also, apart from suckling, not only involves exertion on the part of the muscles of the arms, but likewise those of the trunk and legs, in fact of the whole body; and that the muscles chiefly used are accordingly developed, will be apparent if we call to mind the untiring way in which mothers, and especially poor mothers, carry their children. I have often been struck with the amount of labour women will undergo in this way, and have wondered that they have not sooner known fatigue. It is another of the many wise provisions of Nature, and in this, as in all things, she rewards those most who follow her dictates the closest. Those mothers who nurse and cherish their offspring, are not only more truly *mothers*, but they have a double reward in that while their children thrive, and thus gladden their hearts, they themselves are also very materially benefited. No woman is so healthy as she who bears healthy children healthily. That would appear to be a tautological truism, nevertheless it is well that it should be borne in mind, for it so often happens that women ascribe illness to, or date them from childbearing, that that most natural function comes to be looked upon as a veritable malady, instead of an important physiological process. This, I believe, is mainly owing to the execrable practices which are so often adopted during childbearing; I refer particularly to those which prevail immediately after delivery, and during what is, or should be, if fashion or selfishness do not step in to stop it,—the period of lactation. But this is digressing. The muscles of the hands, arms, back, and neck, then, as well as those which carry the body, are engaged in the acts of nursing and suckling.

There remains for consideration in relation to pregnancy, another muscle, and that the most important of all,—viz., the heart. There can hardly be a doubt, though the physical demonstration of it may be somewhat difficult, about the increase, slight it may be, of this viscus during

pregnancy. Since my thoughts were first directed to this subject, and my observations led me to the conclusion that the heart became somewhat increased in the gravid state, it has come to my knowledge that others have clearly established this position, though at the time I was not aware of it. Indeed, if there be any truth in the foregoing observations, it follows that the power of the heart must be augmented during the gravid state, and this can only be accomplished by means of its muscular tissue. The importance to the economy of this augmentation of the heart's power is abundantly clear on very slight reflection.

Some increase of the muscular tissue of the heart during pregnancy being admitted, the important questions,—Does it, after delivery, return to its previous condition; and, if so, by what means?—naturally suggest themselves. It is a fair inference, and, indeed, one which is confirmed by observation, that the cardiac tissues revert to their former state as soon as the necessity for their augmentation no longer exists. The means whereby this is brought about appears to me to be a most important question. I am not prepared to discuss the subject in its entirety, but briefly it suggests this question,—Is the process by which the heart reverts to its former state the same as that by which the uterus returns to its condition prior to impregnation? viz., by a process of fatty degeneration? or, if this be not the ordinary method, may it not occasionally be so, and thus lead to serious changes in the muscular structure of the organ? This form of retrogressive metamorphosis, as is well known, is perfectly normal in the uterus after delivery. Under what conditions it obtains in other muscular tissues is clearly a question of interest in this connection, but one which can hardly be discussed here, though obviously it has very direct and practical bearings upon the point under consideration. In the parturient uterus the work of involution is carried on by this process until the organ has arrived at the dimensions proper to the non-gravid state, when ordinarily

it is arrested, leaving the womb in a suitable condition for the due performance of all its functions.* But not unfrequently it happens that a premature arrest of the process leaves the womb unnaturally enlarged and bulky, in the condition known as sub-involution, the fertile parent of many uterine troubles. More rarely the opposite condition obtains, and, the process of involution going on to excess, the uterus is left permanently diminished in size, and, according to Sir James Simpson and others, in a state unfitted for impregnation.

Now, it is fair to ask, indeed, one can scarcely avoid enquiring, whether the heart, in resuming its condition prior to pregnancy, ever overshoots the mark in like manner? I have no evidence—at least none that can be stated categorically—adducible in proof of this, but very little doubt exists in my own mind that some of the distressing sudden deaths that occur shortly after delivery, in the midst of circumstances otherwise apparently perfect, are more justly attributable to this condition than to "thrombosis" or "embolism," conditions the occurrence of which I do not dispute, though believing that they are rarer (that is, as sole and primary causes of death) than is commonly believed.

It is not suggested that death from too rapid or excessive involution of the heart after delivery is the commonest, or the only mode in which death suddenly approaches post-partum, but that, in the absence of other very obvious causes, the fatal issue is to be sought for in this direction rather than in "thrombosis" or "pulmonary embolism," conditions which may indeed exist, but are as reasonably attributable to heart-failure, if not more so, as to some obscure coagulation, or other mischief elsewhere. In corroboration of this view, the opinion of Dr Tyler Smith may be quoted. That distinguished physician, in his "Manual of Obstetrics," page 545, says:—"To the list of causes of sudden death occurring in the puerperal

* The work of reconstruction which is simultaneously carried on need not be considered here.

state, must be added fatty degeneration of the heart. I have known several instances in which no other cause of death than a fatty state of the heart could be assigned."

The subject is one which might be pursued much further, but as much has already been said as is consistent with the limits of this paper, which aims to be suggestive rather than exhaustive.

We have seen, then, that the cardiac, and nearly the whole of the voluntary muscles are increased by childbearing, and this process may truly be said therefore to "influence the muscular development of women."

And now one may ask,—Is all the tissue change involved in this muscular increase to be regarded as naught? Far from it; not only is there an increase in the waste and repair of muscular elements, but other tissues also undergo great and corresponding nutritional changes.

Thus the nervous elements presiding over the nutrition and action of the fibrous tissues appertaining to, and probably the bones, etc., to which are attached, the muscles engaged (not to speak of the blood and lymphatic changes which must occur) are all more or less developed in accordance with the physiological law already alluded to.

It is scarcely necessary to add that the preceding observations (with the exception of those that apply to over-involution of the heart) refer exclusively to healthy childbearing women, whose conditions of existence are in every respect normal. Excessive use of organs or tissues will cause them to waste. Bad hygienic conditions and abnormal modes of existence, such as are unhappily but too common, are fertile sources of enfeebled health, and the lowered vitality which results from this will fully account for the numerous cases seen where childbearing is attended by emaciation and other evidence of serious damage to the organism. But such persons are

not healthy, and procreation in them is attended by evils rather than blessings.

And now, some may be inclined to ask, what is the good of all this? A little reflection will, it is trusted, afford a sufficiently satisfactory reply. Without entering upon a consideration of the hygienics and therapeutics of childbearing, it may be remarked that a knowledge of the fact, that a certain amount of muscular development is essential to the wellbeing of a childbearing woman, and of especial importance at the time of parturition, will surely teach us to secure the desired condition, when, from any cause, we have reason to believe it to be wanting. Everyday experience teaches us that those women bear children most easily and safely who lead lives of activity and industry; whose mode of life is, in truth, the least artificial; that, in fact, those who work are the healthiest, provided always that their occupation be not unhealthy, nor its calls upon their strength excessive.

Now, there are many women in a civilised state like ours, who, for obvious reasons, cannot engage in manual labour, and in whom accordingly muscular development may be deficient. But there are no reasons why such persons should not develop their muscles in a moderate degree by calisthenic or gymnastic exercises; and pregnancy is not, necessarily, a bar to such exercises, provided they be properly taken. Of course, the utmost caution should be observed; and, doubtless, skilled supervision would be desirable. A proper course of such exercises would tend to develop, not only the voluntary muscles engaged, but also the heart, and anything that strengthens that organ against the time of parturition, is to be looked upon as a gain, for as a sound heart is always essential to the wellbeing of the body, so is it most essential in the parturient condition, for feebleness then is of serious import, and may lead to a train of evils which, when unhappily they do occur, are too often ascribed to other causes. The value of a strong heart,

then, in the lying-in time cannot be over-rated, and I believe much may be done in the manner indicated to ensure it.

In this way, then, our knowledge of the influence of childbearing on the muscular development of women may be made practically useful.

In conclusion, it may be remarked that same graziers and butchers are aware that gravidity promotes the development of muscular tissue in cattle; and the former occasionally make profitable use of this knowledge.

Dr Gordon remarked that the author, strange to say, had forgotten to mention the influence that childbearing had upon the uterus.

Dr Cuthbert observed that, as a rule, women who had large families got weak and flabby in the belly.

Professor Simpson said that there could be no doubt that the use of any muscle increased its development; and in pregnancy it was not uncommon to find the heart enlarged.

NEW INSTRUMENT FOR RUPTURING THE MEMBRANES.

EXHIBITED BY DAVID GORDON, M.D.

NOVEMBER 23, 1870.

DR GORDON exhibited to the Society a new instrument for rupturing the membranes, which could be fitted on to the exploring finger.

Drs Bruce, Cuthbert, Ritchie, Moir, and *Mr Furley* made some remarks.

ON INCISING THE CERVIX UTERI, AND THE INSTRUMENTS USED AND REQUIRED FOR THE OPERATION.

By Alexander Keiller, M.D.

December 14, 1870.

Dr Keiller communicated the opinions he entertained regarding incision of the cervix uteri, and exhibited several new forms of dilating and cutting instruments he had invented and tested. He spoke of the necessity for the operation in various conditions of the os and cervix, causing painful or abnormal physical obstruction—such as mechanical dysmenorrhœa from preternaturally small os uteri, contracted cervical canal, or flexions or versions of the uterus, forming valvular and other obstructions from displacements. He detailed his experience of the frequency of painful menstruation from mechanical causes, and cited the statistics compiled by others in regard to the condition exhibited by the os and cervix, and referred to the fact that, in addition to contraction and flexure, the cervix was sometimes found to be long and conical, narrow, pointed, and hard, requiring operative treatment to relieve the consequent dysmenorrhœa, or, it may be, consequent sterility. Dr Keiller then spoke of mechanical dilatation, and exhibited the instruments he generally used for dilating the os, and showed the mode of their application and use.

The object of the present communication being more especially intended to demonstrate the comparative advantages of incision over dilatation, and to exhibit to the Society a new form of hysterotome, Dr Keiller entered at some length on the advantages and the disadvantages of the various instruments recommended for incising the os and cervix. The history of the hysterotome, as first used by Simpson, was referred to; and the plans introduced and suggested more recently by others were commented on and compared with the hysterotome and special modes of operation adopted and now recom-

mended by Dr Keiller, who, in conclusion, remarked that, notwithstanding all that had yet been said, thought, or done in regard to this practical and important subject, and especially in regard to the operative treatment of sterility, the results of instrumental interference, if the truth was known or duly admitted, will be found by no means so encouraging as some would have us to suppose, or the apparent efficiency of our dilating and cutting instruments may lead us to expect. Like many others, he had almost daily occasion to treat sterile women, who were usually not unwilling to submit to any operative measures recommended, as at all calculated to ensure their conception, but beyond relief to the dysmenorrhœa, failure was the rule, and the desired success the exception.

Professor Simpson said he was astonished to hear Dr Keiller say that he generally used no other instrument as a sound than the one he had exhibited, for it often happened in practice that a much finer probe was required, and a flexible one was usually desirable. He, however, agreed with much that had been said in regard to division of the cervix. In cases where there is congestion and contraction of the cervical canal, we were formerly warned against employing cutting instruments, but he had in some such cases divided the cervix, and the result was that sterility was cured and the congestion relieved. He was inclined to think that Dr Keiller had rather exaggerated the dangers of Marion Sims's operation. Dr Keiller's operation was more suited, in his opinion, to cases of anteversion. He had himself, in anteflexion of the uterus, divided the anterior lip with a pair of scissors, but not with any successful result. What is wanted in a number of cases is division of the sphincter, and he did not doubt that Dr Keiller's plan would often prove of much service.

Dr James Young mentioned a case in which, after being married for four years without falling in the family-way, a lady had the cervix divided by Sir James Simpson anteriorly and posteriorly, and the result was that a child was born. The cervix in that case was congested, but there was no hæmorrhage of any consequence.

Dr Thomson said that he had patients in whom the cervix was divided, but he had never seen sterility cured

by the operation. He had often seen dysmenorrhœa relieved by it, however, and never heard of any bad results following the operation. Indeed, he had seen worse effects from the use of a sound.

Dr Keiller, in reply, said that *generally* he did not require to use any other sound than that now exhibited, of which, however, he had, as a matter of course, smaller sizes for the purpose of managing cases of more than ordinary constriction; but the instrument shown was really a dilator as well as a sound, and thus afforded a double advantage. He had tried Sims's operation, but was inclined to think it was more dangerous and less required than the simpler operation he had suggested.

Mr Pridie mentioned a case in which sterility was removed by the operation.

CASE OF PUERPERAL CONVULSIONS TREATED BY CHLORAL.

By R. C. Furley, Esq., L.R.C.S.E.

December 14, 1870.

On the 9th October, at seven o'clock in the evening, I was sent for to Mrs R., æt. 18. She informed me that she had sent for me with a twofold object in view. First, she had been uneasy all day, and desired to know if she were really in labour; and secondly, if it were so, that I might be aware of her condition lest I might be sent for during the night. Her appearance gave no indication that her confinement was nigh. She had no look of albuminuria about her, and conversed quietly. She thought she had pains, and, whilst I was examining her per vaginam, said she was suffering from one; but so little was the os dilated that it apparently made no difference upon it. I got the tip of my finger through the os, felt something rounded and hard, and concluded that it was a head presentation, but in what position I could not make out. I felt her pulse to ascertain if the circulation were quickened, as is usual immediately previous to and during delivery. The pulse was 108. Whilst I was holding her

hand, I felt several twitchings something like subsultus tendinum, but of a slight character. The thought passed through my mind that I should not be surprised if the case turned out one of convulsions. So impressed was I with this idea that I wished to remain with her; although, beyond the pulse, there were no indications of immediately approaching labour. My patient, however, with a consideration rarely met with, would not permit me, unless I could say I was of opinion that she would be delivered before morning; which, under the circumstances, I could not do; and it being far from prudent to communicate my suspicions, I unwillingly took my departure. At 5 A.M. on the following morning I was roused by a furious ring, and very quickly learned that my patient had been seized with a fit. On my arrival I was informed that she had been sitting on a chair, talking in her usual manner, when she was attacked. Fortunately the nurse was beside her, and prevented her from falling, which, in all probability, would have seriously aggravated the case. She was immediately put to bed, and in about twenty minutes she had a second fit; shortly after which I proceeded to examine the condition of the os, which examination was seemingly the cause of a third. When the fit had subsided, I put her under chloroform and completed the examination. The os was considerably dilated, and the head presenting in the first position. By waiting a little I felt certain that the forceps could be applied; before this occurred, however, she had a fourth fit. I should mention that the pelvis did not seem roomy, but not so narrow as to shake my confidence in the success of the forceps. The first blade went easily home, but the second could only be brought to within half an inch of the lock. They were taken out and re-applied with the same result; four several times was this done, and neither Dr Milne (who had kindly come to my assistance) nor myself could succeed in getting the blades locked. We were thus reluctantly compelled to resort to craniotomy. After her delivery, of course, I hoped she would have no more con-

vulsions; and, for a time, this hope seemed to be realised, for she had no more for two hours, when she had two within fifteen minutes of each other. After the lapse of another hour she had other two at the same interval. From this time she had two every half-hour, until she had twelve after delivery, making, with the four previous ones, sixteen in all. I had now to abandon the idea that the convulsions would cease naturally, and resolved to administer half-drachm doses of chloral, combined with an equal quantity of acetate of potash—the latter being given with a view to diminish the albumen, which was present in small quantity in the urine. After the first dose she slept calmly for three hours and a half, when she had two fits within a quarter of an hour. Another dose was then given, and was repeated every four hours, but the convulsions did not return, and she subsequently made a good recovery. This case, I think, clearly shows the advantage accruing from the administration of chloral in cases of puerperal convulsions. It might be said, and with some semblance of truth, that the fits were about to cease at any rate. But I would argue against that supposition the fact, that they did not cease even after the chloral was given, but returned after its sedative effect had partially worn off. The success of chloral in this case justifies a further trial, even although it was not so eminently serviceable as the treatment proposed by Dr Bowstead of High Wycombe—viz., the subcutaneous injection of morphia and aconite, under which, in two cases, the convulsions *immediately* ceased. His plan, too, possesses this advantage over the administration of chloral in the ordinary way, that it can be employed whether the patient can swallow or not—a fact sometimes of very great importance.

In connection with this case I ascertained that her mother, and her mother's sister, had both had convulsions with their first children, which would seem to point to a hereditary nature of the disease.

On the subject of puerperal convulsions, I trust that

the Society will deem it of sufficient importance (or at least will pardon me if I do so) to notice one or two points with regard to the frequency of the disease in Edinburgh. As the opinion of one individual can be of very little consequence on such a question, but an opinion based on the experience of the Members of the Society would be of very great value, I venture to put the question—What influence, if any, does locality exert on the production of this distressing complication? I have the statistics of 214,663 cases of labour, and out of that number there were 347 cases of convulsions, occurring prior to, during, or after parturition, and embracing the three forms of the disease—the hysterical, apoplectic, and epileptic. Now, since I commenced practice, I have had in all eight cases of puerperal convulsions, all of the epileptic type. According to the statistics already mentioned, and taking the average of one case of convulsions to 618¾ labours, it would represent attendance on 4950. But I have not had half that number. Is my experience, then, exceptional? Or is there in Edinburgh one case of convulsions in every 300 cases of labour, or thereabouts? In the discussion on a former paper which I read before this Society, describing the treatment adopted in five cases of convulsions, Dr Rosa said he had seen five cases, and Dr Sidey had seen twenty-five. Now, according to the average already given, Dr Rosa should have attended 3090 cases, and Dr Sidey 15,468, which seems such a large number to be attended by a gentleman comparatively young, that I am induced to think that locality really has some influence in the production of this disease, and that there are more cases of convulsions in proportion to the numbers of labours in Edinburgh than elsewhere. The subsequent discussion on this point, I trust, will determine this; and if we can only get at the fact that there are more cases on an average, we may afterwards inquire why it is so. And this question is made all the more interesting, as statistics show that the disease is much more common in England and Ireland than in France.

NOTES OF A CASE OF PUERPERAL CONVULSIONS.

By J. P. Bookless, M.B., C.M., Kelso.

December 14, 1870.

I send you a few notes of a case of puerperal convulsions, which, perhaps, you may consider interesting enough to lay before the Society. I think they show that, however beneficial venesection may be in limiting the force and frequency of the paroxysms, its effects are considerably enhanced by the subsequent administration of chloroform. Not the most unimportant feature in the treatment consisted in the trial of chloral *versus* chloroform in affecting the post-partum convulsions; the complete and permanent effect of the former, compared with the temporary benefit of the other, invoking a decision in its favour.

The subject of the narrative had reached her full term of gestation. The os was about the size of a florin when I first saw my patient at 11 P.M. on the 23d of October last, soft and dilatable, accompanied with regular pains at intervals of twenty minutes; quiet, steady pulse, and as well as possible under the circumstances. She was a primipara about middle age, tall, well-formed, neither florid nor the reverse. As the perineum was still rigid, and no probability of a hurry in the case, I obtained permission to go to bed, which was accordingly done. I considered it perfectly unnecessary to give minute directions to the attendant as to the proper time for stirring me up, experience having oft convinced me how utterly useless the longest-faced directions on these occasions are, despite the anxious protestations on the score of want of sleep for any amount of nights. I had been allowed to doze, however, for about three hours, when I was suddenly roused by one of the women, who was afraid my patient had taken a fit. There was no doubt of it: there were the dilated pupils, the agitated eyes, the spasmodic contractions of face and limb, the hissing

and almost suspended respiration, and foaming mouth. The fit was just going off. Then came the calm, but it was one of total unconsciousness. It lasted for about ten minutes, during which I was glad to find the os almost completely dilated, the vertex just entering the pelvis, and the perineum cool and moist. There was no induration, nor any traces of callosities in any part of the vagina. The membranes had ruptured. As the bed was most awkwardly situated (as I shall afterwards explain), two mattresses were placed on the floor, to which she was at once removed. This being my first case of the kind out of 420 deliveries, I resolved, before employing anæsthesia or venesection, to try the effect of facial dashings of cold water, which Denman seems the first to have employed, with so great an amount of primary success as sufficed to increase his mortification, when, as he tells, subsequent failure convinced him of their uncertainty in checking the paroxysms. Not meeting with success, I now ran to my chloroform bottle with mingled feelings of delight and confidence; but I must confess I was disappointed, the benefit which did result from its exhibition for a considerable time, and in considerable quantity, not being sufficient to realise the hopes I had entertained regarding its potency in such circumstances. Within an hour she had had several strong pains and three paroxysms, the latter being synchronous with the uterine contractions. Thinking it advisable to have my instruments at hand, I sent off a messenger for my forceps, with a note to my father to come and assist if possible, as the friends were all anxiety. Meantime, knowing the general opinion on the point, and remembering Ramsbotham's warning against relying on chloroform solely, to the exclusion of venesection, about ʒxx. of blood, as nearly as I could guess, were withdrawn from the arm, and chloroform again put into requisition. The attacks still maintained their frequency, but their force was now considerably diminished. The uterine contractions continued regular and strong, and a dead

child was at length born, without instrumental aid, about 6 A.M.—half an hour after my father's arrival. The placenta came away five minutes afterwards, and the binder was applied. A paroxysm occurred immediately afterwards, followed by another in twenty minutes; two others occurred within an hour: for all of which latter convulsions chloroform was employed in a pretty large dose at the moment when the first tremor betokened the approach of an attack. Altogether, she had had fourteen paroxysms. She was still unconscious; pulse 120; teeth firmly clenched. By means of firmly wedging a spoon-handle between the teeth, I experimented upon her capability of swallowing with a little water and another teaspoon, and, meeting with success, gr. xxx. of chloral hydrat. (which I fortunately had about me), with two teaspoonfuls of brandy and water, were at once administered. No fits occurring during the next two hours, other fifteen grains were given, and I then left her under the charge of a competent nurse who had meantime been sent for, giving directions to repeat the fifteen grains every three hours, and look to the darkening of the window and proper ventilation of the room. When I saw her again late in the afternoon, she was perfectly conscious, and, upon the whole, comfortable, her only complaint being a peculiar uneasiness over the forehead. I was informed by the nurse that, a short time after I had left her in the forenoon (about twelve o'clock), she had opened her eyes and asked for a drink. The bladder had been emptied also, which was more than I had expected. Her pulse was 80, and gentle,—everything favourable for a good night. Cold cloths were to be kept applied to the head, and a warm bottle to the feet; one more dose of chloral to be administered at bedtime (*i.e.*, about ten o'clock). I saw her again early next morning. She had slept well, but still felt the frontal uneasiness. Lochia natural, and pulse good. Everything went on well for a day or two; bladder and bowels attended to; when, one morning—the fourth after delivery—she began to com-

plain of great abdominal tenderness, necessitating the application of leeches and poultices. On the sixth day, this was considerably relieved. That evening, the nurse informed me that no urine had been passed for twenty-four hours, and that she had not been able to do so. A vaginal examination was immediately made; no clots were discovered, but the meatus urinarius was swollen and exquisitely tender to the touch. About a pint of rather dark urine was withdrawn, and warm fomentations applied to the vagina. For three days the catheter was required morning and evening; and the pulse being good, though weak, she was put upon good beef-tea and a mixture of nitre and iron. On the tenth day after delivery all urethral tenderness had disappeared, and the instrument was dispensed with. On the thirteenth day she rose for the first time, and is at this date perfectly well.

Remarks.—Regarding the causes of eclampsia, much has been said and written; and I have carefully gone over the condition of my patient previous to her lying-in, without being able to call to remembrance any symptom which might stand as a predisposing clue to the serious complication which attended labour. During the whole term of pregnancy she was in capital health. In the course of the last few weeks I frequently saw her, and always found her in the best of spirits. The alimentary tract was always in sound, working order, and no complaints were ever made of vertigo or cranial throbbings; nor had I ever the faintest reason to suspect any nervous irritability of constitution generally. I have also obtained the very important information from one who has known her all her life, that, even when of most tender years, she had never suffered from any of the epileptic ills that infantile flesh is heir to. At a loss for a tangible predisposing cause, it would have been a matter of satisfaction to have been able to determine a tangible exciting one; but, here again, the veil of obscurity is interposed. The soft, dilatable, and cool condition of the os and external parts, in conjunction with the rapidly advancing labour,

are to be considered before launching any direct theoretical abuse against the uterus. She had exhibited no symptom of anasarca at any time; but the first urine I could obtain, after delivery, was found to be highly albuminous, which, though a probable exciting cause, is by no means a necessary one, as statistics fully corroborate. As to atmospherical influences, I must mention one—not in the category of M. Dugès—to which I am inclined to pay a little attention. The chamber was tolerably large, with a rather low ceiling; but the bed was stowed away in a corner, where the ceiling sloped down to the very inside of the bed, making quite an acute angle with the plane of the bed's surface. Add to this the consumption of a goodly share of oxygen by two attendants and a large lamp, with closed door and window, and an idea will be formed of the breathing-room of my patient; and another, as to the probability of the depraved atmosphere acting as a convulsive excitant upon a person peculiarly predisposed—I say necessarily predisposed, as want of space is by no means an uncommon state of affairs in country districts. With regard to the administration of chloroform previous to and after venesection, I have nothing to add beyond alluding to very much more decided results in the latter instance when compared with the former; and it would be interesting to know the experience of the Members of the Society in this particular. At a time when chloral is literally in everybody's mouth, the results following its exhibition in this instance, when given as a post-partum sedative, were eminently satisfactory. To many who, regarding the frequency and force of the paroxysms to be in exact proportion to the length of time the child remains *in utero*, advise operative measures as soon as practicable, the half-hour which was allowed to elapse after the arrival of the forceps might call for censure; "for," in the words of Dr Burns, "the patient is suffering from a disease connected with the state of the uterus, and this state is got rid of by terminating the labour." On this point, amongst the

older writers opinion differed, Baudelocque and others preferring to withhold interference when nature seemed capable of discharging her own duties. With the paroxysms in abeyance, parts dilatable, and uterine contractions strong and effective, as in the case narrated, I think that leaving it to the *vis medicatrix naturæ*, so far, was preferable to following any fixed routine of operative practice. The abdominal tenderness and vesical affection which supervened are the *bêtes noires* which older writers believed to be necessary evils; the words of Denman, regarding the former particular, being—"In almost every case of convulsions that I saw in the early part of my practice, there was evidently, after delivery, a greater or less degree of abdominal inflammation." It is almost unnecessary to say that, in the present day, although this affection does occasionally occur, the idea is not entertained that the one necessarily entails the other. A most interesting phenomenon connected with the subjects of puerperal convulsions obtains in the peculiar condition of mind under which the patient labours, which seems to shut out all remembrance of occurrences previous to the invasion of the paroxysms. On this point, Dr Ramsbotham quotes a wonderful case which occurred in the practice of his father, where the patient became the subject of convulsions on the Tuesday, and, when consciousness returned that evening, remembered nothing which had occurred since the previous Wednesday. My own patient had no recollection whatever of seeing me the evening I was sent for, although she herself gave me permission to go to bed, and chatted most pleasantly with her attendants. She could remember distinctly having ascended the stairs that evening, but had no recollection of anything beyond this, not even of going to bed.

A few observations were made by *Drs Pattison* and *Cochrane* on Messrs Furley's and Bookless's cases of convulsions and their treatment.

Dr James Young then said he had used chloral largely, and found it most serviceable as a hypnotic, inducing sleep

and relieving pain. In delirium tremens it was especially beneficial. In one case which he had seen, it was of the greatest use. The patient was violent and unreasonable, and had not slept for several nights. He was induced to go to bed, and got one drachm of the hydrate of chloral. He slept several hours, and awoke comparatively well. At night he took another drachm, and on the following day went to business as usual. Dr Young said chloral was of immense use in cases of severe nervous headache, illustrated by the following case of a lady resident for a short time in France. She wrote him, complaining of intense headache, with constant wakefulness; chloral was administered repeatedly, and, to use the patient's own words, "the relief was inconceivable." In cases also of infantile convulsions, Dr Young had found it very beneficial, in doses of two grains. In epilepsy, likewise, as evidenced by the following case:—Margaret D——, aged 18, was seized with convulsions when six months old, and had sometimes as many as twelve in a day for twelve years, more or less. At fifteen the seizures became epileptic, and continued for two years, her faculties having been much impaired. I saw her in February 1870, and found her almost an idiot. The mother stated that she scarcely ever slept, and that the bromide of potash had now no effect. The girl menstruated once at seventeen, and once again at eighteen, and regularly thereafter. She would not answer any question, not even tell her name. Doses of chloral were given twice a day, allaying the convulsions and inducing quiet sleep. She has had half a drachm (3 ss.) every night until now (February 1871), with the following results:—M. D—— has had only twelve convulsions during the year. She now sleeps well, answers to her name, knows her parents, walks through the house, and is able to go out occasionally. She asks for her chloral every night. She still has a peculiar expression of countenance, but is much more intelligent.

FŒTUS BORN ALIVE, WITH CUTICLE PEELING OFF.

EXHIBITED BY JAMES A. SIDEY, M.D.

JANUARY 11, 1871.

DR SIDEY exhibited a fœtus, which had been born alive the previous day, and lived about ten minutes. The

cuticle at birth was peeling off, as if the child had been dead for some time. He thought the case was interesting medico-legally. The placenta was contracted on its fœtal surface, and round the margin.

PLACENTA WITH TWO CORDS.

EXHIBITED BY JAMES YOUNG, M.D.

JANUARY 11, 1871.

DR JAMES YOUNG showed a large placenta with two cords. In this case he said the first child was still-born, the cord having a knot on it close to the abdomen, which he thought was sufficient to have caused its death. The second child was born alive, cord natural and healthy. The lady told him that the fœtal movements had been comparatively weak for the three weeks previous to her confinement.

PRACTICE IN THE PREDICTION OF THE DAY OF CONFINEMENT.

BY J. MATTHEWS DUNCAN, M.D.

JANUARY 11, 1871.

IN this brief paper I propose to pursue a purely synthetical line of argument, in order to show the accuracy of conclusions regarding the best mode of predicting the day of confinement which I had arrived at by a different process, and which I published in the *Monthly Journal of Medical Science* for March 1854.

The following were the chief results then stated, with a view to the ordinary calculation in practice:—

That the average interval between the end of last menstruation, or extending from and including the first day after the cessation of last menstruation, to parturition, is 278 days.

That the day fixed upon by this calculation is not the natural day, but only the most probable day of confinement; and that, in consequence of its variations, it is better to avoid predicting any day, but to predict the occurrence of childbirth in a week of which the said 278th day is the middle, or very nearly so.

Since these conclusions were published in 1854, there has been considerable discussion of the subject both at home and abroad; and there can be no doubt that there is a general tendency among scientific inquirers to advance in the direction which I followed,—that is, tending to show that the average duration of pregnancy is shorter than older authors generally supposed.

Many still very erroneously write, and, to a great extent, reason as if the date of conception could be made out, and as if the date of a fertilizing coitus were the date of conception. It is surely unnecessary for me to go over this ground again; for not a single argument is adduced in support of these views, and they are known to be not only not demonstrated, but to be not in accordance with our positive knowledge.

The most elaborate recent paper on this calculation which I know is by Dr Ahlfeld, and is published in the *Monatsschrift für Geburtskunde* for 1869. His theoretic conclusions differ from mine chiefly in reducing the period of pregnancy from 275 to 271 days. But I am very far from being satisfied with his data, especially with his mode of getting assurance as to the date of insemination, or, as he erroneously calls it, conception. He trusts, in my opinion, far too much in the mere statements of the females. So much is this the case, that I am disposed still to adhere to my own figure of 275 days as the nearest approach to a correct statement of the average duration of pregnancy.

Dr Ahlfeld further tries to show that the majority of women are confined in the 39th week of pregnancy,—a statement quite in accordance with his previous conclusion regarding the duration of pregnancy, and, I need not add,

not in accordance with the view of the duration of this state to which I adhere.

But prediction of the day of lying-in is an important practical matter, from whose arrangement all theory should be excluded. It is a valuable calculation of a quite empirical kind. Its successful performance does not necessarily depend at all on correctness of views as to the duration of pregnancy.

We cannot count from the beginning of pregnancy, or conception, as Ahlfeld pretends to do, because in no case do we know the day or the week in which it begins.

We cannot, except very rarely, count from a single coitus, or coitus only on a single day, because such circumstances seldom occur, and because, even when they are alleged to have occurred, we can very seldom obtain satisfactory assurance of them.

We almost invariably count from the last menses. The end of last menstruation is generally taken as the point to count from; and this is a rational proceeding, because cohabitation is, as a rule, suspended during the flow, and the female is not liable to be impregnated till after it has ceased. But, as I have already said, the calculation is purely empirical, and might, as is actually done by Cederschjold and Berthold, be made from the beginning of menstruation just as well as from the end of it. I adhere to the old plan because it is the old and generally-used plan, and because, therefore, the data from which the method of calculating the day of confinement has been elaborated have been made out by it. Had we more numerous and more carefully collected data, based upon a system of counting from the beginning of menstruation, I should be ready to give up the old one and take the new one. Both systems yield the method of calculating on purely empirical, not on rational, grounds. Authors have committed grevious errors in vainly trying to combine empirical and rational grounds for this calculation. In the present state of science this is impossible. Only confusion can arise from so doing. There can be no

objection to authors deriving evidence from this calculation for or against propositions in science; but at present science can lay down no grounds for the calculation other than the records of experience. Ahlfeld is the ablest representative of such attempts, but in practice he comes in reality to simple dependence on the date of last menstruation.

In my former papers, to which I have already referred, and which form part of my work on Fecundity, I use the great recorded experiences of Merriman and Reid. Estimating by these, I find that the 278th day after the end of last menses is the average day of delivery at the full time; and on this I proceed. No ingenuity can devise a superior plan of estimating, so long as the last menstruation forms the only generally available *terminus a quo*. The introduction by many authors of scientific views into the question of the best way of predicting the day of confinement, may be justly characterised as either at least unnecessary or else merely pedantic. Till I find a larger and more carefully compiled mass of facts, than those of Reid and Merriman, I shall adhere to my method of calculating based on the circumstance that 278 days is the average interval between menstruation and parturition; and in doing so I have science and common sense on my side.

The method which I recommend is confessedly a rough one. The calculation itself is always what is called a rough one. My method certainly is loose and erroneous to the extent of one day in certain cases, which I have specified at page 340 of the first edition of my work on Fecundity already referred to. The plan is simply as follows:—Find the day on which the female ceased to menstruate, or the first day of being what she calls "well." Take that day nine months forwards as 275 days, unless February is included, in which case it is taken as 273 days. To this add three days in the former case, or five if February is in the count, to make up the 278. This operation is perfectly simple, and so easy of performance as to render a periodoscope quite useless.

Now, any practitioner can test this plan by his own experience, in a purely synthetical and reverse manner. He can try the plan, and then see how it has led him; whether it has led him and his patients into error or not. Since I adopted this reverse method of verifying my plan of calculation, I have found that Ahlfeld had already resorted to a similar test. It is only very slightly different from the method by which the plan of calculating was developed. The difference is stated as follows:— Cases of delivery collated yield results on which the plan is founded; instances of prediction compared with the real events test the plan.

I shall now show what my predictions on this plan have come to. No one can hope to be an absolutely good prophet in this matter, but we can be as good as possible, as nearly right as may be. The predictions to which I shall immediately make reference were all written down before the events, and remain written. I have only 153 cases to refer to, all collected within several recent years. They are few, because I did not venture on the written-down prediction unless I was satisfied that I got good information as to the day of the cessation of the menses.

I need scarcely repeat, that in practice I do not predict a day, but a week. I predicted a day in my note-book for my own use. These 153 predictions in my note-book I now analyse.

In 10 cases the day of confinement was exactly predicted, or about once in every 15 cases.

In 80 cases the confinement took place sooner than was predicted. The number of days of anticipation was, for the whole 80 cases, 590, or an average of above 7 days for each case.

In 63 cases the confinement took place later than was predicted. The number of days of protraction was, for the whole 63 cases, 535, or an average of above 8 days for each case.

In 63 cases, or more than one-third of all, the time of confinement was successfully though not exactly predicted,

the birth occurring not earlier or later than 4 days from the predicted day.

The average error was about 7½ days,—a circumstance which indicates that the prediction should not state the week of confinement but the fortnight of confinement, there being generally an error of a little above 7 days on the one side or the other of the ascertained average day.

But the most interesting result of these figures is the answer to the question, Can the calculation be improved? and the answer is, that it is, for practical purposes, perfect, or as nearly so as the present state of science permits. This near approach to perfection is shown, firstly, by the observation, that the errors on either side of the predicted day are nearly equal. If the errors on either side were exactly equal, then the calculation would be perfect; for it would thus be shown that, for the mass of cases, the exactly most probable day of confinement had been hit upon. In my 153 cases, the excess of error is on the side of anticipation. This excess is 55 days. Now, 55 days for 80 cases is less than a day of average error; and as our prediction does not pretend to even the accuracy of a day, the error may be truly regarded as trivial.

There is another, far more precise, and the only true way of analysing these or like results with a view to ascertaining whether the calculation I propose is the least erroneous possible. This method consists in ascertaining, not the average error on each side of the true point, but in observing the amount of error in each successive day on either side of the true point. This method, pointed out to me by Professor Tait, has been kindly also carried out by him, and I here give his note containing the details of it.

This note is of some value, even in connection with the small number of cases which I have for analysis by this method, which is founded on the theory of probabilities. Were my cases ten times as numerous, it would enable us to arrive at final results. But I make careful mention of the method here, chiefly because of its extreme value as a suggestion for the use of future investigators. When

applied to a sufficient number of instances, it forms the only exact means of testing any plan of calculating. It not only tests such plan, but gives, when worked out by

FIG. 1.—Showing Dr Matthews Duncan's data Graphically.

the method of plotting used by Professor Tait (*see* fig. 1), a correct view, at a glance, of all the errors in defect or in excess; and not only this, but also, with equal facility, a correct view of the importance of the errors. In addition to all these advantages of this method, which, so far as I know, has not yet been applied to the subject on hand, there is another, that, from a sufficient number of observations exactly made, it will enable us to elicit with certainty the true plan of calculating. It will not only show errors in an old method, it will show also how to avoid them—how to correct the old method.

The first figure here given is a mere ocular view of the errors. It puts the variations in my 153 cases not in a new light so much as before a new sense; not before the eye in written words, but before the eye in represented masses.

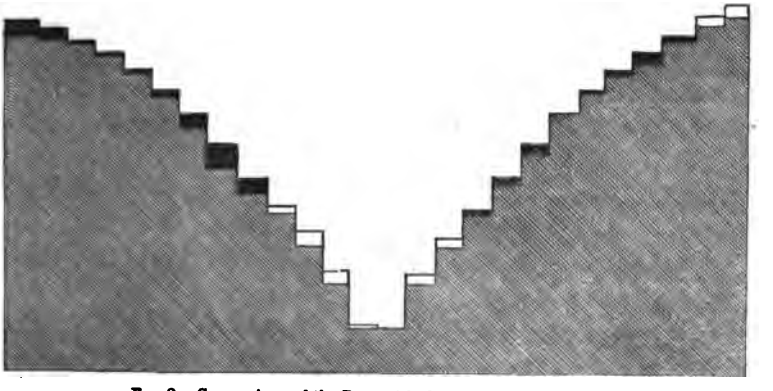


FIG. 2.—Comparison of the Data with the ordinary Law of Error.

The second figure is the important one. It requires a little explanation. The black and white parts are the errors which ought not to be—errors arising from imperfections in the plan of calculating. There must, of course, be many errors, in one sense, in these predictions of the day of confinement; but did the analysis of my 153 cases show no black and white, my plan would then be perfect. All avoidable error would then be eliminated. A different plan of calculating might be discovered, but the present plan could not be farther improved. All this can be demonstrated by the laws of probable error.

If my method of calculating were perfect of its kind, the figure would have no black and white. In order to recognise how it would then stand, the student must not merely erase the black and white parts, and put the gray in their place. The white or blank represents excess, and white must be simply erased—the white parts entirely removed from the figure. But black represents defect; black parts, therefore, are not simply erased or removed from the figure, but the black is erased, and the general gray colour put in its place. The figure, as it then would be, gives the correct amount of error—the inevitable error.

The general appearance of the figure shows that in my 153 cases of prediction, the amount of avoidable error is small. It gives at once such a view of the avoidable errors as would be very difficult and tedious to put into words.

Accumulation of cases will soon lead to the easy elaboration by this method of an absolutely correct method of calculating the term of a period whose length is indefinite.

"Your data," says Professor Tait, "though numerous in the sense of having been collected from your own observations, are rather scanty for the application of mathematical methods. I have, therefore, confined myself to a very simple species of interpolation, which seems to be sufficient to extract from them their most important contents.

"When the numbers are plotted, as in the first figure, we notice some strange irregularities, the most singular of which are actual *minima*—seven and fourteen days before, and eight and fifteen days after, your typical period. What these may mean (if they are *real*, and not due to mere defect of data) I cannot conjecture. If we suppose them due to defect of data, as I have no reason to doubt, there is still the curious fact that the errors in excess of the period are not merely more numerous than those in defect, but *they extend farther in time*. This must, I feel sure, be due to miscalculation on the part of some of the patients.

"By a tentative process, I find that all your numbers, irregular as they at first sight appear, with the exception of those last mentioned (which, for the reason given, I consider myself entitled to reject), accord fairly enough with the ordinary law of probability of error, provided we assign, as the true period, the second day before that given by your rule. Thus we obtain the following series, which is graphically represented in the second figure, white representing excess, and black defect of observation as compared with calculation:—

Days.	1	2	3	4	5	6	7	8	9	10	11	12	13	
Within.....	16	35	50	59	68	71	85	97	105	111	116	118	119	etc.
Beyond.....	15	34	47	56	65	76	90	98	104	108	115	125	130	etc.
Calculated.	15	30	44	57	69	80	90	99	106	112	117	121	124	etc.

The second and third lines are found for separate days by adding together each successive pair of your numbers, and the fourth is roughly calculated from the ordinary tables of Probability of Error. It would be easy to make the coincidence more exact, but the labour of the necessary calculation would hardly be justified by the extent of the data."

I shall now briefly compare some of my analytical state-

ments with similar statements regarding the data of Ahlfeld, made in the elaborate paper already referred to.

Ahlfeld predicted the day of confinement by his own method in 1014 cases, of which he has the details in 915 instances.

In 30 of Ahlfeld's cases the day of confinement was exactly predicted, or about once in every 30 cases. My success was twice as great, the prediction in my cases proving exactly true about once in every 15 cases.

In 205 of Ahlfeld's cases the time of confinement was successfully though not exactly predicted—the event occurring not earlier or later than four days from the predicted day. This was a success in much less than one-fourth of his cases. My similar success was in 63 cases, or more than one-third of the whole.

Further, Ahlfeld points out that 465 of his cases, or less than one-half, showed not above 11 days of error in the prediction. Of my cases 120 showed not above 11 days of error in the prediction, or considerably more than two-thirds of the whole.

It is thus seen that, so far as the limited number of cases can show it, my plan surpasses Ahlfeld's to a great degree.

I may add that, with a view to comparing his own plan with Naegelé's, Ahlfeld calculated (not predicted) the day of confinement for 258 cases, of which he possessed all the necessary details, including, of course, the day of confinement. He found the average error to be, for his own method and for Naegelé's, nearly 10 days. Mine was only 7½. His own method proved a little more accurate than Naegelé's.

Naegelé's plan is to fix upon the seventh day from the first of the last menstrual period, and to predict the same day of the third next month, counting backwards.

Ahlfeld's plan seems to be to fix upon the seventh or eighth day from the beginning of menstruation as the day of conception, and to add 271 days.

Before concluding, it is necessary not to omit mention

of a correction of one of my own practical recommendations. I say that the accoucheur may venture to predict the week of confinement, or to fix upon a day which is the middle of the week in which a woman is to be confined. Now, as the average error is about 7 days on each side of the event, it is evident that the accoucheur should not predict confinement in a certain week, but in a certain fortnight, or fix upon a day which is in the middle of the fortnight in which a woman is to be confined.*

Dr Thomson said he had met with three cases in which, after a single coitus, confinement took place at the end of 273 days.

Dr Duncan stated that some time ago he had collected the particulars of a number of such cases, and the conclusion he then arrived at was that 275 days was the time, and it was very much the same in cases where women dated from the day of marriage. The tendency, however, amongst authors seems to be to shorten the period. It was rather curious and interesting to know that in the calendar the time between the Festival of the Annunciation and Festival of the Nativity was exactly 275 days.

THE FUNCTION OF THE PERINEUM IN PROCIDENTIA UTERI.

By J. Matthews Duncan, M.D.

January 11, 1871.

The causes of prolapsus of the uterus are very imperfectly known. Various circumstances which predispose to it, or more directly produce it, are generally described. Among these are childbirth, diminution of the retentive power of the abdomen, hard work, or violent exertion, or any other cause of ordinary hernia.

But of late years, especially since the extensive recom-

* Spaeth, Compendium der Geburtsk., s. 71, states the ordinary range of error as extending over 14 days.

mendation of operations upon the perineum as a means of cure of the morbid condition, procidentia has been ascribed to laceration of the perineum. Most modern authors on the diseases of women do more or less prominently adduce this laceration as an important cause.

My object in the present paper is, *first*, to show that laceration of the perineum is not, in any strict sense, a cause of prolapsus or procidentia; *second*, to point out what influence laceration of the perineum exerts in this disease; *third*, to show what is the value of restoration of the perineum.

I. *Laceration of the Perineum is not a cause of Prolapsus or Procidentia Uteri.*

No one, so far as I know, has explicitly ascribed to the perineum any part in the maintenance of the uterus in its natural position. This is very remarkable, considering the great combination of authorities that can be adduced, who, by ascribing procidentia to laceration of the perineum, thus evidently imply that it has some power to maintain the uterus in its natural site. To prove that it has no such power, I rely upon the following arguments:—

In several cases of complete destruction of the perineum, the laceration extending backwards through the whole of the sphincter ani, which have come under my notice, and on which I have operated, there has been no example of prolapsus of the uterus complicating the laceration; and the great majority of them has been observed in poor, hard-working women. The perfection of laceration has been present, and its influence has been supplemented by those of childbirth and hard work, and, notwithstanding, prolapsus has in these cases not taken place.

I have seen a large procidentia in a young girl, who had never menstruated, whose perineum was entire, and whose hymen was not ruptured at its posterior part.

Cases of this kind are not extremely rare. The perineum is perfect, and yet the accident occurs.

Numerous cases have come under my notice, in virgins and in sterile women, where the integrity of the perineum did not prevent procidentia.

In the majority of the numerous cases of procidentia which come under my notice, the perineum is either entire, or not more injured than it is in the great mass of women who have borne a child.

I have no doubt that if, by way of experiment, the perineum was cut through in a healthy women, no tendency to prolapsus would be thereby produced.

In judging of the state of the perineum in cases of procidentia, it is necessary to guard against a great source of delusion. In all there is, at first sight, an appearance of destruction of the perineum. Its antero-posterior dimension is always greatly curtailed, and this even in cases where it is perfectly entire, and where the hymen can be seen to be only injured and changed, not divided. The large bulky procidentia pressing on the anterior margin of the perineum distends it, softens it, and pushes it back towards the anus, destroying its antero-posterior dimension, much as the body of the child does after the birth of the shoulders.

II. *The Influence of Laceration of the Perineum in Procidentia Uteri.*

While I have already given good reasons for believing that the perineum has nothing to do with the maintenance of the uterus in its natural position, and that laceration of it has no causative influence in the production of procidentia, there can yet be no doubt that laceration of the perineum favours or accelerates the occurrence of procidentia in cases where the causes of this accident are in operation. The study of this point brings out the function of the perineum in this disease.

The uterus, morbidly propelled through the pelvis by the causes of prolapsus, comes at length to press upon

the perineum, slightly distends it, advances along it, reaches the orifice of the vagina, protrudes through it, and is at last procident. It is evident, at a glance, that if this long course through the pelvis and over the perineum to the vaginal orifice is in any way shortened in its latter part, the sooner will the procidentia occur. To arrive at this stage of procidentia, the uterus has to go through a shorter course, and is therefore earlier procident. Now, laceration of the perineum abbreviates the latter part of the path of the uterus just described. It does not produce prolapsus—that is owing to quite other causes; but it abets such causes by removing difficulties which otherwise would have to be overcome. These difficulties lie not only in the length and resistance of the perineum, but also in the smallness and tightness of the vaginal orifice.

A comparison with some of the phenomena of natural labour appears to me to be both just and illustrative. Just as the perineum is not a source of any of the forces which concur to produce the birth of the child, so it is in procidentia of the uterus. But though this is the case, its laceration makes the birth of the child more rapid and more easy; it secures a shortening of its course and an easy exit through the enlarged vaginal orifice. Just as a woman with laceration of the perineum will have the progress of a prolapsus to complete procidentia accelerated and facilitated in its latter part, so a woman in labour, who has previously had an extensively lacerated perineum, is liable to a precipitate birth.

III. *The Use of Restoration of the Perineum.*

I do not purpose here to describe the operation of renewing the perineum. I shall merely say that it is an operation I have frequently performed with much advantage. My object is to give what I believe is the correct theoretical statement of its use and value. There are other surgical appliances, especially the padded T bandage and the vaginal pessary, which are also of great use and

value in the treatment of cases of procidentia. To these I shall have to refer.

It is a great mistake to suppose that a perineum, however renewed, removes any cause of prolapsus or procidentia. The causes of that displacement are all still present. The perineum is restored in order to resist the progress of the descending uterus. It may or may not succeed in doing so. This will depend on the force with which the uterus is propelled, and the force which the renewed perineum is capable of offering in resistance to it. In many cases the resistance offered by the new perineum is insufficient; it yields, and the uterus becomes again procident. The birth of a child may relax the renewed perineum without lacerating it, and thus, destroying its resisting power, may lead to a return of the procidentia; or, repeated childbearings may take place before the return of the procidentia.

Restoration of the perineum is not a cure of procidentia. It deserves in some sense this name only when its rigidity is such as to be sufficient effectually to oppose the progress of the uterus trying to force its way over it. This opposing power may be increased by the pressure exerted on it by the pad of a T bandage; or even a pessary may be further called in to assist in keeping back the propelled uterus. If one resistance to the advance of the uterus is not sufficient, another may be tried, or a third, or their forces may be variously combined to produce the desired result.

Here, again, the analogy of childbirth is very exact and illustrative. The child, like the prolapsing uterus, is propelled against the perineum. If the perineum is rigid, or if the propelling force is not great, the progress of the child may be arrested by the strength of the perineum. But, just as in most cases of prolapsus, continued propelling impulses at last cause the child's head to distend the perineum, and burst through the vaginal orifice. On the other hand, the powerful palm of the accoucheur pressing on the perineum may be sufficient to arrest the progress of the child. In the same way, the pad of the

T bandage supporting the restored yet too weak perineum, in a case of prolapsus, may be a sufficient addition to the resisting power of the perineum, and co-operate in effectually arresting the descending progress of the uterus.

The T bandage may not be efficient without restoration of the perineum, because without this there may be no part appropriate to receive the pressure of the pad of the bandage. The pad may be inefficient when pressing against the vaginal orifice, from the want of the aid of the restored perineum, and from the inappropriateness or inefficiency of the pressure. The advantage of having a perineum to receive the pressure of the pad of the T bandage, I have illustrated repeatedly to my pupils by the following rude analogy, in which the door stands for the renewed perineum, the intruder for the uterus, and the pad of the bandage for the owner of the house. It will be easier for the owner to keep the intruder out of his house, if his efforts are merely required to keep a door shut, than if there were no door, but an open passage; and this is true, even though no aid is got from the fastenings of the door, such as is got from the power of a renewed perineum.

Lastly, it is easy to understand that that method of restoring the perineum is to be preferred which ensures to it the greatest degree of strength or rigidity.

Dr Keiller said that in cases of procidentia the weaker parts gave way; the perineum could have nothing to do with maintaining the uterus in position as the womb was so high above it. In his opinion the causes of procidentia were pressure from above downwards, relaxation of the uterus, and increased capacity of the vagina. Thus, in cases of ruptured perineum, the uterus, instead of having to find its way down through a curved canal, passes directly through a straight one. He agreed with Dr Duncan that procidentia might occur without any rupture, as in the case of virgins, but in such cases he believed that the falling was generally the result of elongation. Women who have been fat and become emaciated, have a tendency to prolapsus, because they lose the padding of

the vagina. He had repeatedly seen procidentia in patients who had been operated on for rupture of the perineum, but the operation was usually resorted to, not for the purpose of curing the prolapsus, but in order to prevent it, and also to allow of the use of a suitable pessary.

Dr Milne remarked, that most authors stated that the perineum affords some support to the uterus indirectly. That is to say, the perineum supports the vagina, the vagina supports the uterus, and hence, if the perineum be ruptured, the uterus has a tendency to come down.

Dr Bell thought that the true cause of prolapsus was relaxation of the uterine ligaments.

Dr Duncan, in reply, maintained that the perineum had nothing whatever to do with the support of the uterus.

CASE OF MALPOSITION OF THE UMBILICAL CORD.

By James Young, M.D.

January 25, 1871.

On the 2d October 1870, I was sent for to see Mrs P., who was about to be confined of her second child. I was informed that she had been complaining for two days, more or less. The case appeared to me to be a very simple one, with natural presentation. The head was well down in the cavity of the pelvis, and a large male child was born two hours after I arrived. My attention was immediately called to an abnormality in the position of the cord, it being tightly twined round the loins, and once round the top of each thigh, leaving a sulcus distinctly marked round the body and legs. I easily extricated the child from the meshes of the cord. The infant's cry was somewhat weaker than usual. I made a most careful examination of the limbs, which were much swollen, as also the scrotum. All the lower half of the body was very hard and exquisitely painful, the child crying when the limbs were moved. The colour

was dark, and the limbs were very cold, and motionless. During the twenty days while the child lived, he never moved either limb, and only slightly the toes. The child passed water abundantly, and sucked well. The position of the limbs was remarkable; each leg and foot being completely turned out, exactly like a skater in the spread eagle position, and were so fixed. The treatment was simple. I ordered the limbs to be fomented constantly, and wrapped in sponge-piline. The child was kept on the attendant's knee constantly for the first week. An oleaginous application was laid over all the lower parts, and in ten days the limbs were somewhat fallen, and the dark colour lessened. The pain seemed so far abated, but the position of the limbs never altered. On the fifteenth day (17th Oct.) the child became much worse, and slept almost none. The limbs were more swollen and much darker, and a slough began to form over the lower part of the sacrum. I asked Professor Simpson to see the child on the sixteenth day (four days before it died).

The case was doubtless one where the current of blood had been almost completely interrupted before birth by the strangulation of the cord, which was sufficient to account for the appearances presented after birth. I have been induced to bring this case under the notice of the Obstetrical Society, as it is the first of the kind which I have seen. I have only to add, that before death sphacelation commenced over the sacrum, scrotum, and both limbs.

Dr Murray suggested that the abdominal aorta of the child must have been imperfect.

Dr Cuthbert said he had attended a case in which the cord was twisted so tightly round a limb as to nearly cause amputation.

Dr Bruce said, that in cases where the cord was tightly twisted round the child, it was the venous circulation that was most affected, hence the infiltration.

THE CONNECTION BETWEEN LACERATION OF THE PERINÆUM AND PROLAPSUS UTERI.

By Alexander Milne, M.D.

January 25, 1871.

I may state that I have been led to glance at this question, from hearing the paper bearing on the same subject read by a distinguished Fellow at last meeting of our Society. In that paper it was denied that laceration of the perinæum had anything to do with prolapsus uteri; and altogether this part of the female, in relation to this displacement, was, in my humble estimation, by far too lightly esteemed. I most certainly believe that the perinæum plays a very useful part, though by no means the most important one, in the way of supporting the uterus; and the object of this brief paper will be to point out this essential function.

Let me add preliminarily, that if the able Fellow's view be the correct one; if the perinæum offers no support, even indirectly to the uterus, then I certainly say the sooner it is swept away the better, just like any other nuisance. I say nuisance, because we, as accoucheurs all, know dearly what a trouble it gives us in many instances; how great a resistance it offers to the descent and exit of the child; what valuable time is sacrificed to its obduracy; and if it be so unimportant a part, we may all pray vehemently for its laceration, and when the great boon arrives, refuse, with an obstinacy equal to that of the perinæum itself, to have the rupture stitched. Nay, if nature, full of ingenious devices, should exert her powers in the way of healing the breach, we shall see to it that her unwelcome efforts are thwarted, and that the happy wound is left to gape as widely as possible. I say this in sober earnestness, and not in a joking, or even sarcastic spirit; for if we meet with an impediment (I don't care in what department), that is an impediment and nothing more, the sooner it is brushed aside the better—"Let it perish as a worm upon destruction's path."

But is the perinæum an obstacle and nothing more, an intolerable impediment without compensatory good; the unmitigated patience-slayer of the doctor? No; it cannot be, for in nature

> "Nothing useless is, or low,
> Each thing in its place is best;
> And what seems but idle show,
> Strengthens and supports the rest."

I know it will be said that the denial of the perinæum, as a uterine prop, does not mean the disbelief in its utility. But I maintain that it does; for it cannot support the uplying, superincumbent viscera, or antagonize the diaphragm, without supporting the womb. If you affirm that it does not support the uterus, then you are tied up to the negation of its function as a counterpoise to the diaphragm; in other words, you reduce it to the low position of a useless appendage; indeed a piece of mere and pure trapping.

But now we must proceed to evidence, to show that the perinæum in its normal state does aid in supporting the uterus; and when lacerated, favours the descent of that organ.

Function of the Perinæum.—The question as to the connection between lacerated perinæum and prolapsus uteri (or procidentia, for they differ only in degree), derives light from a consideration of the function of the former; what is that function? The perinæum, according to all authors, has a twofold office to fulfil—1. It has to dilate during parturition, as we all know practically, and independently of a knowledge of its elastic structure. 2. It antagonizes the diaphragm, and supports the superincumbent viscera of the pelvis and abdomen. It is with this latter function that we have mainly to do in connection with the subject under consideration. The perinæum then supports the pelvic viscera, one of which is the uterus. True, it does not touch the latter, being some inches away from it, and stress was laid by some on this circumstance; but if it is to be argued that it

cannot support the uterus, because it is some distance from it, then we may also argue that the copestone or apex of a spire is not supported by the foundation, because it is removed a very great way from the base of the edifice. The key-stone of an arch is also a good distance removed from the foundation, but how long would the bridge remain aloft were this latter cut away? The uterus we maintain then is supported by the perinæum as a part of the mass of elastic viscera lying above and between it and the diaphragm. The whole abdominal and pelvic cavity is a closed box, containing organs of a somewhat soft consistence, and elastic and distensible qualities. These organs are bound together by various ligaments and other connections, but not in such a way as to render them independent of their encircling walls. From these walls they derive support, and if any part is destroyed, or even weakened considerably, a protrusion of some part takes place. We see this illustrated in the different forms of hernia, and in wounds of the abdominal wall. Moreover, if the opening be situated in the lower part of the cavity, this protrusion is all the more likely to occur, and to a larger extent, because the contained viscera are subjected to a deal of vertical pressure, and which is all the more concentrated as we approach the lower part of the cavity. This perpendicular force consists of the action of the diaphragm, and the numerous muscular efforts called into play in walking, in lifting heavy burdens, in coughing, in ordinary, and more so in strained defæcation, and in ordinary and forced micturition. The voluntary and involuntary forces exerted during successive labours, also form part of this vertical force. So long as the antagonist of this diaphragmatic, and other force, remains whole, and of normal strength, it offers a powerful resistance to it; but when it is weakened by morbid causes, or by labour, and much more so when it is ruptured,—when it has the line of insertion of its different muscles torn through,—its antagonising power is diminished or destroyed. If the laceration

is slight, little evil may result; if extensive, say everything involved but sphincter ani, much evil may accrue. If this muscle is also severed, the resulting mischief will be greater still. Prolapse of the contiguous viscera—the rectum, the uterus, and the bladder—follows, with all their attendant distress. These results are not fanciful, but are testified to by the gynækologists with a unanimity that compels our respect, if it does not secure our ready and willing assent.

Bearing of Ruptured Perinæum on Vagina.—But the perinæum supports the uterus in a more direct manner than it does as an antagonist of the diaphragm; that is, through its intimate connection with the vagina. The uterus, of all the internal organs, is characterised by a remarkable mobility ("Es ist ein sehr bewegliches Organ, er streift umher wie eine Zigeunerin," as I have heard a German say), and may be made to alter its place in all directions until its supports are put upon the stretch, as we may see by making traction on it. Well, the vagina is one of these supports; the uterus rests on it as on a firm though elastic pedestal, and receives from it no little support indeed. The vagina, in turn, is supported by the perinæum, and if its fascia and muscles are torn through, the posterior vaginal wall is undermined. One can only deny the support rendered to the uterus by the perinæum, owing to its connection with the vagina, either in the first place by denying that it supports the vagina; or, secondly, by denying that the vagina sustains and props the womb. No one will deny the former—that is, that the perinæum supports the vagina (unless, indeed, some curiosity of a being, like the doctor in the north, who denied Harvey's splendid discovery to his latest breath), so those who see no relation between laceration of the perinæum and prolapsus uteri, must deny the latter,—that is, that the vagina is one of the buttresses of the womb. They are shut up to the latter course, an unsatisfactory position indeed, and for this reason. It can be, and has been demonstrated, that the vagina aids in

supporting the uterus. When the ligaments of the uterus, broad and round, are cut through in the dead subject, and downward traction is made on the uterus, much resistance is opposed by the vagina and its connections, and considerable force is required to produce a prolapse. Evidence such as this is convincing, nay, irresistible; one fact is worth a tome of fancy. Those who underrate the perinæum as an antagonist of the diaphragm—as a prop and stay to the superincumbent viscera—are compelled to resort to other explanations of the manner of maintenance of position of those organs. For example, Dr Duncan has invented a hypothesis which he has termed the "retentive power of the abdomen," an account of which will be found in the *Edinburgh Medical Journal* for December 1865, or in the able author's work, entitled "Researches in Obstetrics," at page 409. The Doctor does not define precisely what this power is; at least he does not explain it sufficiently to my understanding. Is it some magnetic power or centripetal force, or just some mysterious and occult something which helps to maintain the viscera in their normal site? Whatever it may be, if it does exist, one thing is transparent, that when you breach the abdominal wall, it has not the power to restrain the viscera, or to hinder them from protruding.

Effect of Ruptured Perinæum on the Direction of the Vaginal Canal.—Further, ruptured perinæum favours prolapsus uteri by destroying the curvature of the vaginal canal, and making it approach to the perpendicular. In the normal condition of matters, the axis of the vagina does not correspond with the long axis of the uterus, but is at a certain angle from it, and prolapsus cannot take place in such circumstances beyond a very limited extent; for, if the uterus came down it would be met by the posterior wall of the vagina, and supported by it, just as is observed in those cases of labour where the uterus, from pointing too much in the direction of the axis of the brim, threatens to drive the child through the perinæum instead of the ostium vaginæ. The only way in fact in which uterine

descent could take place with the vagina normal, would be by the uterus becoming retroverted. The case is entirely altered, however, when the vagina loses its curvature and becomes vertical, it is then no longer capable of arresting the descent of the uterus, for its axis is continuous with that of the pelvic brim, and thereby a straight and facile avenue is opened up for the prolapse of the womb. It may not enter on the downward path, owing, say, to the strength of its superior attachments; but it can hardly avoid doing so when the road is so easy, and when, like a heavy goods train, there is an engine pulling in front and another pushing behind. The engine behind is the diaphragmatic force and that of the viscera lying above the womb, and that in front is the traction exerted by prolapsed contiguous viscera, rectum, or bladder.

Effect of Lacerated Perinæum on Bladder.—When the perinæum is torn extensively during labour, the vaginal orifice is rendered too ample instead of being firm and close. Usually the canal is obliterated by the pressure of the neighbouring viscera, the bladder anteriorly and the rectum posteriorly, and by its own tonic contraction, under normal auspices, so to say. In this occluded state it is a firm pillar to the womb, but now it is expanded, and in this yawning condition of the canal, and of its orifice, the bladder loses needed support, and gravitates gradually until a cystocele is established. This cystocele, from the intimate connection between the bladder and anterior vaginal wall, and betwixt this latter and the cervix uteri, almost infallibly exerts traction on the uterus, and draws it down, until ultimately a procidentia is produced. Thus a lacerated perinæum, by leading to prolapsus of the bladder, favours also prolapsus of the womb.

Influence of Ruptured Perinæum on Rectum.— If the bladder gravitates abnormally on being deprived of the support yielded by the vagina and perinæum, so does the rectum. Indeed, it is rather more apt to do so, as may be seen by making a female with lacerated perinæum, or even simply

with a very relaxed vagina and perinæum, bear down with as much force as she can command. In such a case, you will notice, perhaps, first the rectum coming down, and then the bladder and uterus. Well, in saying that the rectum is prolapsed, it is almost tantamount to saying that the uterus has descended, for the one cannot gravitate far without dragging the other along with it, so close is the connection between them. Thus, a prolapsed rectum due to a defective perinæum, is another cause of uterine descent.

Evidence derived from the Prolapse of the Aged.—Further light is shed on the relation of lacerated perinæum to uterine procidentia, by the prolapsus common to females of advanced years. It is common to find this displacement of the uterus in elderly women, where there is no hypertrophy of that organ, but rather, on the contrary, where there is atrophy, and consequently diminished, instead of increased, weight of the womb. Moreover, where there has been no exciting cause of an unwonted nature, such as that produced by physical exertion or straining. I have met with a few of these cases. How are they to be explained? Just in this way. There is an absorption of adipose tissue, and consequent shrinking of parts. The fat of the perinæum disappears, its muscles attenuate and become feeble, and it fails to afford the vagina adequate support. The ostium vaginæ also gapes widely from the wasting of labial fat; the result being that the natural curvature of the canal is done away with, and it becomes, as in rupture, more vertical, and its axis in approximation with that of the long axis of the uterus. Thus there is again an easy passage for uterine prolapse. The flabby vaginal walls also support the bladder very inefficiently, and a cystocele may be established, and help to drag the uterus down.

Prolapsus in the Young and in Virgins.—All that we have hitherto advanced has been in favour of the view that the perinæum has to do with the support of the uterus; but now it behoves us to notice some evidence apparently in

conflict with this doctrine. This is derived from the cases of nuns, or other virgins, who have been affected with prolapsus, notwithstanding that their perinæum has never been in the way of rupture, has never in fact been torn. There are also some few cases of children who have suffered from a descent of the uterus, and who have not of course suffered from a laceration of the perinæum. We have all known or heard of sturdy women able to go about, too, with their perinæum fissured, and yet having no prolapsus. These instances would therefore appear to stagger one a little; but yet our belief in the supporting power of the perinæum is not in consequence shattered, and for diverse reasons. (1.) We have seen that a perinæum, even though unruptured, if flabby and feeble, may favour prolapsus; and these nuns and other virgins may have had degenerate perinæal muscles, and scant store of fat, in the right place, that is to say. (2.) In those sturdy females who peregrinate with a split perinæum, and yet have no procidentia, there is usually hypertrophied tissue set up by nature as a substitute. (3.) The perinæum is only one of several props of the uterus, and not at all the most important one. No, the causation of prolapsus uteri is a wider affair (as we could have shown, had we been specially treating of the subject), its causes are numerous, because the womb counts numerous stays. Displacement need not follow the enfeeblement of one of these supports, because the others may be unusually strong. In one case, the prolapsus may be precipitated by weakness of the inferior supports; in another, by feebleness of the superior ones. Nay, more, a procidentia may be caused even when all the supports of the uterus are marked by their usual strength. In those conventual females, and other virgins, with prolapse without lacerated perinæum, the cause may have been an abnormally short, straight, and relaxed vagina, or a feeble perinæum; or a morbid or congenital debility of the utero-sacral, and broad and round ligaments; or there may have come into operation some of those various accidents, such as lifting weights, straining at stool, coughing,

jumping, etc., which involve much perpendicular force. Indeed, this explanation is rarely awanting in such cases. In the case of a child, such as the one mentioned by Monro, there may have been congenital defects to account for the displacement.

Besides, one or two cases will not prove a rule; or, if they did, we might say that menstruation began in infancy, and the power of conception extended to 72 years of age, or further. If it can be shown to us that cases of prolapsus are as numerous in virgins as in married and childbearing females, then our faith in the view we have been advocating will be powerfully shaken; but this is not the case. All gynækologists look upon the virgin cases as exceptional, and not very common, and that the bulk of them occur in those who have been exposed to the disorders of pregnancy, and the accidents of labour.

Such are the principal arguments in favour of the importance of the perinæum in relation to the maintenance of the womb in its position; and of its rupture as one of the causes of its displacement. I might have given extensive quotations from numerous authors in support of the opinions I have advanced, but space would not permit. Any one, however, may satisfy himself of the fact that there is wide testimony in favour of these views, by perusing the better known obstetric works. I trust that in this brief paper I have at least helped to vindicate the value of the perinæum,—a value which at our last gathering was somewhat depreciated,—and thus, perhaps, help to avert practical evil. For, if we hold too cheap any particular part, we are not likely to be so careful in conserving it; if we lightly esteem the perinæum of the female, we may not view its laceration as any great matter, nor the healing of it any great necessity, if necessity at all.

Dr Bell was of opinion that the perinæum is not a very important organ in the support of the uterus. The principal structures concerned in the support of the womb are the vagina and uterine ligaments, especially

the round ligaments. This is shown by the fact of the right round ligament being shorter than the left, in order to guide the uterus to the right side. There may be a ruptured perinæum without procidentia. It is doubtful if the perinæum can be of much use in counteracting the pressure downwards of the diaphragm, for in straining there is no tension of the perinæum. He had seen some cases in which the perinæum had been restored by operation, and the uterus came down notwithstanding.

Dr Ritchie alluded to the paper he had communicated from Dr Madden, with whose views the Society appeared to agree. Amongst other reasons for advocating the immediate treatment of rupture of the perinæum, one was to lessen the chance of prolapsus occurring, and another to permit the patient, if so afflicted, to wear a pessary. He was rather surprised, therefore, at the view the Society apparently took at the last meeting, and at that expressed regarding the function of the perinæum. He thought it was only reasonable to hold that where all the structures were intact there was less likelihood of prolapsus taking place. Hence, if the support of the vagina is taken away, the parts external to the uterus will become relaxed, and a tendency to descent of the womb favoured. Consequently he could not but regard the perinæum as aiding the ligamentous supports of the uterus.

Dr Gordon remarked that teachers usually pointed out that the perinæum afforded support to the uterus, as one of the pelvic organs.

Dr James Young said he agreed with the remarks made by Dr Bell. He remembered two cases where there was rupture but no prolapsus. He had also seen prolapsus in unmarried females, where the perinæum was quite tight.

Dr Cuthbert said he had met with cases where there was a good deal of prolapsus, and, at the same time, great narrowness of the vagina.

Dr Murray was of opinion that the retentive power of the abdomen must be weakened by rupture of the perinæum. He had seen cases of rupture without procidentia. The perinæum served to keep the vagina in a rigid state.

Dr Bruce thought that the perinæum had, to a certain extent, the power of supporting the uterus.

SHELL OF AN UPPER INCISOR TOOTH, OBTAINED FROM AN INFANT BETWEEN FOUR AND FIVE MONTHS OLD.

EXHIBITED BY ROBERT BRUCE, M.D.

FEBRUARY 8, 1871.

DR BRUCE exhibited the shell of an upper incisor tooth, which had been obtained from an infant between four and five months old, after suffering from a deep-seated abscess, which pointed over the situation of the incisor, and was opened. A considerable quantity of matter was evacuated, and it continued to suppurate for a day or two, when the tooth just dropped down from its socket; it was some weeks before the suppuration ceased, and the parts returned to their normal condition. It is not uncommon to meet with a small collection of matter and blood over a tooth shortly before it finds its way through the gum, but the peculiarity in this case was the deep-seated nature of the abscess (the tooth not being near the surface), and the complete absorption of all parts of the tooth except the enamel, there being nothing left but a mere shell.

CASE OF PARALYSIS AND SUDDEN DEATH IN THE PREGNANT STATE.

BY R. PEEL RITCHIE, M.D.

FEBRUARY 8, 1871.

I DESIRE to submit this case to the Society solely on the grounds that it is no common one, and that it has been the only one of the kind I have met with, complicating pregnancy.

I can claim no merit for my accuracy of diagnosis, nor for the success of my treatment. Placed in the circum-

stances I was when called to see the case, I do not think, on reviewing its course, I would, even now have acted differently; and seeing the result was fatal, I have considered it proper to place it on record, to call the attention of Fellows of the Society to a formidable form of disease complicating the pregnant state. It is to be greatly regretted that the feelings of the friends would not permit a post-mortem examination. In the medical certificate of cause of death, I found it therefore necessary to state that the patient was pregnant, suffered from paralysis, that the death was sudden, and that no post-mortem examination was permitted. I did not expect that the Registrar would have been satisfied with a certificate so worded.

15*th December* 1870.—Mrs S., aged 25.—A message was left at my house last night at a late hour to call on this patient. As I did not receive the message till midnight, in consequence of being professionally engaged, I did not call till to-day. I found her complaining of severe pain in head—all over it, but chiefly on left side. So severe was the pain that she could not move the head without screaming out. I received the following history:—She had previously enjoyed good health, but stated she had always been very "nervous." Had been married for six years. She was now pregnant for the fourth time, and in previous pregnancies had enjoyed good health; her labours were easy, and her recoveries satisfactory. She was taken ill about three weeks ago with a pain in the left side of head, and also apparently with a pain in left shoulder, and states she was treated for rheumatic fever and neuralgia; has been unable to sleep from pain in the head; for some time has passed daily, and still continues to pass, very little water. About two weeks ago, lost power to move left arm; the left leg soon after became affected, and recently the right arm became partially paralyzed, but the right leg has still retained its motor power; there has been anæsthesia of left arm and leg for some days, but for how long is uncertain; the

joints have not been swollen. Has felt no fœtal movements for several days.

On examination, she is a stout, previously healthy-looking woman. She complains of breathlessness; cannot lie except on left side. She lies almost on the abdomen, and the uterine tumour is felt displaced in consequence. She cannot move herself. The respiratory sounds are normal; the voice is clear, and she makes frequent outcries of pain. Heart-sounds are also good, except perhaps a slight prolongation of the first sound, but there is no bruit. The pulse beats at 80, and is regular and of fair strength. The appetite is tolerably good; she requires to be fed. The bowels are confined. No "globus" described. The tongue is coated, indented, and drawn to left side. She passes very little water at a time, and only with great difficulty. Her desire to do so is urgent, and she requires to be often got up, from her frequent ineffectual efforts to micturate She is moved with difficulty, from being so heavy, and unable to assist in moving herself. The urine is loaded with red urates (to be examined); it has been scanty for some time—not exceeding a pint a day. There is no swelling of the feet, but the face on right side looks somewhat swollen. The skin is not hot, and she perspires, but not to any excessive extent. Considers she has completed 6½ months of her fourth pregnancy. The os is high, and has an irregular feeling; the point of finger can be introduced. Supposes some escape of the liquor amnii occurred within the last day or two. Has felt no fœtal movement for some days. On examination, no fœtal heart-sounds are heard, but from her position in bed examination is difficult.

She is quite unable to move herself; requires to be lifted into and out of bed. There is tenderness along the course of the spine. Has no feeling in left arm, but it is warm; she cannot move it. Can tell the toes of left foot when touched, and can move right leg. The feeling in and motor power of right arm are impaired. Has constant

pain in head on left side. The left cheek looks flattened. The left eyelid is opened to less extent than right, but whether from ptosis or from pressure on it I am uncertain, though inclined to regard it as a state of ptosis. Pupils are equal and contractile. Right side of face full and flushed; there is an expression of suffering.

As she was taking a saline diuretic mixture several times daily, and morphia at bedtime, I made no change in treatment until I examined the state of the urine.

16*th December*.—Passed very little urine yesterday, and I only got some late last night. On examination, find it is albuminous, but only to a slight extent, there being little more than a trace. She had a bad night, even with the morphia. Ordered Pil. col. co. immediately, and 10 gr. Bromide of potass every sixth hour.

Midnight.—Was called to her, as she was stated to have labour-pains. Found there was no action of the uterus going on. She had frequent calls to micturate, but had passed no water for nearly twenty-four hours. At 2.30 A.M. introduced the catheter, and drew off about 10 oz. of high-coloured, but not ammoniacal, urine; she then said she felt much easier; has been sweating.

17*th* (Forenoon).—Past a tolerable night after I left. The bowels acted freely; left arm no better; less sensibility in left leg; less motor power in right arm; can still move right leg. Catheter used—10 oz. of urine were drawn off.

18*th* (Forenoon).—Passed a good night, but no urine; less nervous power all over body; head less pained; no fœtal movement. From her position in bed, had difficulty in introducing catheter; about 10 oz. were drawn off.

Evening.—Heard fœtal heart sounds this evening. She takes the bromide every four hours, and a teaspoonful of Sp. æth. nitrosi twice daily.

19*th* (Forenoon).—Voice not so loud (partial aphonia); can now lie more on back; there is ptosis of left lid. Is unable to move left leg, or right arm or leg, but has

returning feeling in and can keep left arm flexed when resting on elbow; pupils contractile, apparently equally so, and of regular shape; fœtal heart heard distinctly. Urine urgently requested to be drawn off, which was done by nurse, but the quantity is not materially increased—she suffers less pain. Professor Simpson saw her with me to-day, and recommended ergot in small doses, in the view there may be congestion of the spine.

20th.—She passed a good night, had even more power in left arm, but the urine was very troublesome, and had been drawn off. She took a good breakfast, and was cheerful. The nurse left her for a short time before 11 A.M., and, on her return, observed a change. She looked dark in colour, and the face was drawn to right side; the breathing was also affected. She was unable to speak, but the power to articulate soon returned. She was then conscious, and soon after became unconscious, and died at 1 P.M., "as the gun fired." I was not sent for to my house, but looked for at the Infirmary, through some mistake on the part of the messenger. Whilst conscious, longed for my arrival, and said she was dying. On my calling in course of the day, found she had died an hour and a quarter before. She was cold; no fœtal sounds were audible. My first thought was to remove the fœtus by section; but considering the stage of pregnancy, and the time she had been dead, I refrained from doing so. Her pupils were dilated; the teeth firmly clenched; frothy mucus, as if she had suffered from a convulsive attack, issued from the mouth; the lips were darkly discoloured, and I failed to force open the jaws. I stated a post-mortem examination would be necessary, but this was afterwards refused.

Remarks.—When I visited this case for the first time, I saw that it was one of neither a trivial character nor of frequent occurrence. I looked for an explanation of the phenomena of the disease first in the state of the urine, expecting to find it in a highly albuminous condition;

but, on the contrary, although there was a trace of albumen in it when it boiled, there was not sufficient to warrant me in concluding that the state of the kidney explained the nervous symptoms. Besides, too, this was not a first pregnancy, and I was warranted, by the opinion of observers, in concluding that if albumen were present, it was most likely due to a chronic condition of kidney disease, and consequently that the presence of albumen would have been shown in larger quantity. This source of the paralysis I therefore dismissed, as being insufficient to account for it.

2*d.* The next view that occurred was, is this one of those cases described as reflex? But on reviewing the whole circumstances of the case, I did not consider that this view would explain the constant neuralgic pain on left side of head, the apparent ptosis of left lid, and the progressive extension of the paralysis from left arm to left leg, to right arm, and subsequently to right leg.

3*d.* Another source of the paralysis which occurred to me was arterial obstruction; but careful auscultation convinced me there was no valvular lesion of the heart, and the progressive character, without sudden commencement of the paralytic attack, induced me to abandon this view.

4*th.* I must own I had some doubt at first whether the attack might not be hysterical. The statement of the patient that she had been previously "nervous," her outcry, and the retention of the urine, her healthy appearance, all favoured this; but I dismissed this view when I became more familiar with the case. The absence of the hysterical globus, the perfect anæsthesia, the ptosis, the scanty renal secretion, all induced me to regard the case as due to some cause more serious, however hysterical the manner of the patient might be.

5*th.* The only conclusion I could come to was, that the source of the paralysis was in the central nervous system, and that the paralysis was to be regarded as a complication, not a consequence of the pregnant state.

The diagnosis I made was, that the patient, whatever other lesion there might be, was suffering from congestion of the meninges of the spine and base of the brain. Adopting this view, I placed her on the bromide of potass, at first to the extent of 40 grs. a-day, and afterwards on 60 grs. As the diuretic mixture she was taking had produced but little effect, although powerful in its composition, I intermitted it; and as the bowels had been costive for several days, obtained a free action by means of compound colocynth pill. As the nursing of the patient was very far from what it should be, I felt it my duty to recommend her to apply for admission to the Maternity Hospital as a private patient; but this was declined, and it was only two days before her death that she was placed under the care of a qualified nurse. This diagnosis does not, however, account for the sudden termination of the case within two hours.

6th. Since her death, when I review all the indications of her state, I am rather inclined to view her disease as having possibly been a local cerebritis ending in abscess, and that the congestive symptoms of the spine and base had been secondary to the local cerebral inflammation. Not having seen the patient in the earlier stages, I cannot state whether the premonitory gastric indications existed, or whether the sickness of cerebral disease might not have been supposed that of pregnancy; but when I first visited her, the confined state of the bowels was repeatedly spoken of by her, and the difficulty there had been to get them to act by means of medicine was alluded to. I certainly have seen one instance verified by autopsy in which cerebral abscess existed and proved fatal, in which, till within six days of death, the symptoms of cerebral inflammation were not distinct, but in which a severe local cephalalgia existed on the left side, and corresponded with a circumscribed intracranial abscess.

The symptoms in Mrs S.'s case which seem to me to favour the view that she suffered from a local cerebritis

are—The headache being the first symptom, and, so far as I can understand, of sudden origin and local in position, becoming unilateral; the ptosis; the intellect being so little affected; the sensibility of the limbs being so markedly affected, whilst the temperature was not diminished; the progressive character of the paralysis; and, in the absence of any disease in the thoracic viscera, the sudden death, which seems to have been preceded by convulsion, followed by coma of a very limited duration, and might possibly have been produced by the rupture of a cerebral abscess. The absence of delirium, the regular state of the pupil, the returning power in left arm, and also there being no rigidity of any limb, may be urged against this supposition; but an ordinary apoplectic attack appears to me to be insufficient to explain the circumstances of her death.

Before concluding, one word may be expected regarding my non-interference on behalf of the fœtus. When I saw the patient was dead, my first thought was to open the abdomen; but reflecting, when I heard that she had been so for at least an hour and a quarter, that there was no probability of saving the child, and even if I had had the chance of operating immediately after death, that it was doubtful if the child would have lived, I refrained from a procedure which I felt would have only been for the gratification of my own curiosity, and could have resulted in no benefit to the unborn child or the afflicted relatives.

Professor Simpson had seen the case so well described by Dr Ritchie. He agreed with him that the lesion must be looked for in congestion or inflammation of the coverings of spine and brain. It reminded him of a case which he had seen some years ago of a lady who was under treatment by the late Sir James Simpson and Dr Begbie for pains coming on subsequently to delivery, and believed by them to be of a rheumatic nature. One evening she was suddenly seized with numbness in the lower extremities, and sent for Sir James, who, being somewhat indis-

posed, requested him to see her. He found the numbness was attended with paralysis, and as it seemed to be increasing in the lower extremities, he sent for both of her physicians. What was supposed to be merely a rheumatic pain proved really to be the result of an inflammation in the lower part of the spinal cord, which suddenly began to extend upwards, and which caused the death of the patient within eight hours after the commencement of the first alarming symptom. He thought Dr Ritchie could not, in the circumstances of the case, have done more, but considered the fatal result might be explained by a sudden extension of the congestion to the medulla, without there being, as Dr Ritchie had suggested, any rupture of an abscess; but in absence of post-mortem examination, speculation was of little consequence. He also thought in such obscure cases that permission for a post-mortem should be obligatory on the friends of those dying.

Dr Cochrane, without reflecting in any way on the management of the case, thought that the treatment might have been more antiphlogistic.

Dr Coghill and *Dr Gordon* also spoke.

Dr Ritchie, in replying, stated the pulse had never been above 80 in frequency, and was not inflammatory in character.

CASE OF PUERPERAL GASTRIC FEVER.

By L. R. Thomson, M.D., Dalkeith.

February 22, 1871.

Mrs C., aged 27; first pregnancy. About a fortnight before delivery felt often feverish; severe headache, and a feverish condition generally in afternoon. Sent for November 3d, 1870. Called twice during the day, and went back at 10 P.M.; was detained all night, and with a short absence in the forenoon of next day, remained until *delivery* at 2 P.M. About to use forceps when *progress* became decided, and child was born in first position. Did

well all that day (Saturday) up till Sunday afternoon, when feverish symptoms began, and pulse rose to 120. Great tenderness over abdomen, over which poultices were applied, and morphia given by the mouth. These symptoms went on much the same all Monday. During Tuesday, and up till Thursday, exacerbations and *fever* occurred between 2 and 5 P.M. The climax in the way of severe symptoms was reached at this date. The patient was now very ill in the evenings, and improved early in the morning. Calomel and opium had been given, and afterwards castor oil, followed by quinine (gr. ij.), three times a-day at first, and afterwards twice a-day. Milk always in breasts, and lochial discharge continued for a month, after which red discharge took place for a week, *i.e.*, during fifth week after delivery. Effervescing drinks were given on account of thirst. Three weeks after confinement there was great pain after food, requiring bismuth, opiates, etc. Aguish symptoms were very marked in this case, rigors, heat, and sweating being much pronounced. The tongue, which at first was coated with a creamy fur, and red at the tip and edges, became brown and afterwards red, with papillæ very prominent. In this last condition, magnesia, lime water, and milk diet were used, along with chicken and beef-tea, etc. The bowels were at one time constipated, at another relaxed, in which latter case the secretions had an ochry, pea-soup colour.*

During the fifth week after delivery, there was retching and vomiting, and consequently much difficulty in retaining food; a condition of things always to be regarded in

* Tympanitic distension of the abdomen was a distressing symptom during the course of the disease, but was always more or less relieved by the persevering use of poultices. In cases of peritonitis and gastric fever, where the abdominal distension is very great, I am strongly inclined, as a *dernier resort*, to try paracentesis abdominis, as I believe in some cases the retching and vomiting are due to mechanical pressure. Of course it must be borne in mind that in this malady the local disease consists of enlargement and afterwards *ulceration* of the agminated and solitary glands of the ileum.

a serious light when occurring in or after the third week of gastric or enteric fever. In this case there was a distinct relapse six weeks after delivery; chiefly owing, I think, to the resumption of erect posture, and the too early use of solid food.

There are great varieties of gastric fever; but in any form of the disease we now know that the disease is due to the existence of a specific poison. In this case I discovered foul emanations proceeding from a fixed wash-hand basin in the room where this lady was confined. As soon as I discovered this state of things, I had the patient removed to another and larger bedroom. If this had not been done, I very much fear I would not now be able to announce that this patient made a good and complete recovery.

Professor Simpson regarded the case as one of typhoid fever occurring in pregnancy, and thought it was of importance to bring that out, as some authorities believed that the pregnant state precluded typhoid fever. He believed that many cases of supposed puerperal fever were really typhoid occurring in a puerperal woman, and alluded to Dr Hecker of Munich's paper on this subject.

Dr Ritchie had listened with much interest to Dr T.'s communication. He observed that he had alluded to the fixed basin as a source of fever poison. He had some time ago attended a good many cases of typhoid fever in one street, and although they were not connected with the puerperal state, he might remark that these cases, in his experience, had all occurred amongst the members of families occupying and sleeping in the rooms in which sinks were fixed. He believed that the soil-pipes connecting these with the drains (which were found to be out of repair) had been the source by which the fever was propagated, and that to a greater extent than by water-closets. He objected very strongly to the insanitary state of such houses, and considered that no sink should be permitted in a sleeping room, which was often rendered more unhealthy by the number of those sleeping in it. Sinks and water-closets were too often introduced into

small houses, and were injurious to the health of those living in them. In such houses he considered if the sink and closet were placed outside in the stair, with external ventilation, it would be preferable. He alluded to Dr Churchill's remarks on gastric and enteric fever in the puerperal state, to its being sometimes epidemic, and to frequency of recoveries.

Dr Murray remarked on the fallacy of the supposition of fever being always fatal in the puerperal state, and asked Dr Thomson his view on this point.

Dr Thomson stated that scarlet fever was not always fatal in the puerperal state, and referred to a case seen by the late Sir James Simpson, who had expressed a hopeful opinion, whilst the other medical attendants took a different view of the case. The characteristic eruption was seen in this case. Sir J. S.'s treatment was to paint the abdomen with iodine—a favourite method with him.

Dr Bell wanted to know for what purpose this was applied.

Dr Thomson replied, to relieve the tympanitis, which was believed to be due to inflammatory action in neighbourhood of womb.

Dr Bell regarded tympanitis as the symptom of most serious import. With regard to the treatment proposed for its relief by Dr T., he did not think that if the abdomen were perforated the tympanitis could be relieved, as he believed the air was within the bowel.

Dr Thomson thought, in extreme conditions, that the bowel might be punctured for its relief.

Dr Bell thought that, where the tympanitis was great in amount, the case was settled. He also said that Dr Littlejohn had alluded in his Sanitary Report of the City to fever arising in the upper flats of houses from their communications with the drains. He also made observations as to the bad state of the Greenside district, where he had visited as cholera inspector, and in which he had found soil-pipes without water, and bad smells coming from them at night.

Dr Bruce remarked as to the danger of tympanitis. He thought the remark of Dr Thomson as to puncturing

was worthy of attention, and referred to a case of cancer he had lately had under his care, where the tympanitis was so great, he would have risked the operation with an exploring needle, if it had received the sanction of the gentleman who saw the case with him.

Dr Thomson, in reply, observed that in only two of the houses at Eskbank had typhoid fever occurred, and in both, the cause had been distinctly traced to the drains; there was scarcely an inmate but was more or less affected by the fever-poison. That it should have occurred in only two houses was the more remarkable, for at Dalkeith, we are never without some cases of typhoid.

CASE OF MALFORMATION OF THE FŒTAL BOWEL.

By Alexander Milne, M.D.

February 22, 1871.

Dr A. Milne showed a preparation consisting of a part of the large bowel of a fœtus which terminated in a cul-de-sac about two inches above the anus. In this case there was an anal orifice, which is not very common where the bowel ends in a blind extremity. This opening was only about half an inch in depth, and did not therefore reach to the gut. The child commenced to vomit some liquid food given it, 24 hours after birth, and the abdomen gradually became swollen, tense, and painful. It succumbed from peritonitis and exhaustion 52 hours after birth. No operation was performed, for the sufficient reason that Dr M. was only called in when the child was moribund, a midwife having been in attendance at the confinement. Doubtless the fact of there being an anal orifice, had led to the delay in seeking professional assistance, the suspicion of some defect only being excited by the persistent vomiting, abdominal distension, and absence of meconium.

Dr Keiller said that this was a rare malformation, inasmuch as there was an anus. Usually in cases of this kind the anus was absent, although raphé was almost always seen. He had operated on one or two cases with the view of making an artificial anus, but the patients died. The great difficulty was to keep the passage open after operation, and with this object it was necessary that the bowel should be drawn down and stitched to the margin of the anus.

Dr Taylor said that while resident in Sutherlandshire, he had frequently had opportunities of seeing similar cases of malformation in sheep. The operation is often performed by shepherds with considerable success. The sheep being a delicate animal, many of them die, but at the same time a considerable number recover. He could not see why the operation should not succeed in the human subject. In such a case as that before the Society, he would be inclined first of all to perforate with a trocar, and afterwards introduce a good sized bougie. Judging from experience in connection with other canals, he considered that there should not be any insuperable difficulty in keeping this one open.

Dr Bruce remembered a case which was operated on by Mr Spence, and the child lived.

Dr Gordon said an artificial anus might be made in the iliac region.

Dr Pattison remarked that it was no uncommon thing to see an artificial anus in hernia cases.

STATISTICS REGARDING VACCINATION, WITH REMARKS.

By Robert Bruce, M.D.

February 22, 1871.

The object of the present paper is to bring before this Society some of the results which have been obtained by me in regard to vaccination, and is intended to show

the amount of success I have met with, and to elicit the experience of other Members in order to arrive at some conclusion as to the best method of vaccinating, and the results which ought to follow the various modes of operating.

The two chief methods, I presume, are by direct transmission from arm to arm, and that by means of lymph preserved in tubes, hermetically sealed.

Other means are made use of more or less extensively, such as by points, equivalent to the old method of preservation between flat glasses; some also make use of the scabs, rubbed down with a little water, or preserved in glycerine.

Then there is a new method which I have heard is being tried just now, viz., applying the lymph to the surface previously made raw by means of a blister, and then removing the cuticle—a method which appears to have nothing to recommend it but its novelty.

One point upon which there seems to be great diversity of opinion is the number of places in which the lymph ought to be inserted; many in London, I believe, consider that at least four pocks are necessary to ensure protection. To my idea, the production of one perfect vesicle will give as thorough a protection from smallpox as fifty would do, and is quite as good an evidence of the system having undergone that change which renders it insusceptible of the contagion of smallpox, and, moreover, that that insusceptibility will continue as long in the one case as the other. It might as well be affirmed that a mild attack of smallpox would not produce so thorough an immunity from subsequent attacks as a severe one would do, and with as great an amount of reason. I think, therefore, that vaccinating in more than two places is a piece of unnecessary severity, and for which there are no good grounds whatever. Although I consider that one vesicle is sufficient, it is my practice almost invariably to vaccinate in two places, to guard against a possible failure in

one of them, and because the production of two vesicles does not usually give rise to any superabundant amount of irritation. If one or more perfect vesicles are not produced, I think it advisable to re-vaccinate soon after; although, in general, no further effect has been produced where I have done so, still it is a satisfaction to have it thus tested. Another question has been mooted, whether it is proper to vaccinate when smallpox is in the house; some, I understand, will not do so for fear of the two pocks meeting. It is unknown to me whether this has any hold of the professional mind, but I have heard of vaccination being withheld on that account. My experience is not great with regard to this, but I may mention three cases bearing upon it, where vaccination was performed, in the hope that the system was not already under the influence of the variola, in the stage of incubation.

One case was that of a child, a year old, which had never been vaccinated; smallpox being in the house, I at once performed the operation, and although the vaccination took very well, the smallpox also appeared, and both ran their course together, while the child did not seem to suffer any more on account of the two pocks meeting. The other case was that of an infant a few weeks old—I think about six weeks; the mother was nursing it herself when she became affected with smallpox. On the first appearance of the eruption, I immediately vaccinated the infant; the effect was extremely satisfactory, as the vaccination succeeded well, and the infant escaped the smallpox altogether, although continuing to be nursed by its mother. The third case was that of a child, which was vaccinated when the eruption of smallpox had just begun to appear on its mother; the vaccination succeeded, and the infant escaped a disease which it would otherwise in all probability have taken. The last two cases also bear upon the point whether two vesicles are sufficient to protect against smallpox, that being the number of places in which these infants were vaccinated.

I will now proceed to give the statistics, premising that nearly all the cases were vaccinated in two places, in one arm only, producing a few scratches in the skin by means of a lancet not too sharp in the point, through the lymph previously applied to the part.

I have vaccinated occasionally in one place only, where there seemed to be any particular reason for doing so—as, for instance, at the request of a timid mother; and have also vaccinated in more places on special occasions—as in a case where I wished to produce obliteration of a nævus, and considered it necessary to surround it with four separate vesicles; but these are exceptional instances.

The cases in the infant which I refer to are 1000 in number, taken from my note-book just as they have occurred to me during the last few years.

In the first place I will include all the cases, whether done from the arm or from tubes, and then divide them into two classes—those done directly from arm to arm, and those done by lymph preserved in tubes.

1. *Both Classes combined.*

Number of cases, 1000.

Age of infants, generally about 3 months.

Successful the first time,	976	In both places, .	924
		„ one place, .	52
„ after second operation, . . .			22
„ „ third „ . . .			2

Total, 1000—all successful.

2. *Vaccinated from the Arm.*

Number of cases, 807.

Successful the first time, ...	801	
In both places, .		778
„ one place, .		23
„ the second time, ...	4	
„ „ third time, ...	2	

3. *Vaccinated from Tubes.*

Number of cases, 193.

Successful the first time,	. . .	170
	In both places, .	141
	„ one place, .	29

Unsuccessful, 22—but were done successfully afterwards from the arm.

One only was done again from the tubes, and with success.

From a consideration of the above figures, it will be seen that vaccination from the arm is decidedly the most successful, and ought always to be preferred if possible; for it will be observed that, out of 807 cases, there were only 6 failures after the first vaccination, while in those done from tubes there were 23 failures out of 193, and every precaution was taken to ensure the preservation of the lymph, while I was careful not to take much lymph from any one vesicle. I observed that, at certain times, it seemed as if there were more failures from the tubes than at other times. The tubes employed were of various ages, but mostly quite recent. I vaccinated five children from tubes a year old, and four of these were successful.

As bearing upon this point, I may refer to the result of some cases of re-vaccination in the soldier, in which it is stated that 3 per cent. only were successfully done from points, 55 per cent. from tubes, and 87 per cent. from the arm.

I have re-vaccinated many adults, both from the arm and from tubes, but have not noted the result particularly. I found that most of them took in some degree, but only a few of them had perfect vesicles; probably it would have been found, had they taken smallpox instead of having been vaccinated, that those having the perfect vesicles would have taken smallpox in a severe form, and the others in a milder degree, corresponding to the more or less perfect nature of the vesicles.

That vaccination is as complete a protection as an attack of smallpox itself, is very well shown by Dr Heim's report:—

Vaccinated with success after smallpox, .	32
„ „ modified, . .	26
„ without effect, ...	42
	100
Re-vaccinated with success, ...	34
„ „ modified, . .	25
„ without effect, . . .	41
	100

The period at which re-vaccination ought to be performed will depend upon the time at which the protective power of the first vaccination ceases; but this can only be determined by experiment, as the period in different individuals varies extremely—with some it holds good for life. I have over and over again vaccinated myself without success; and I have attended a girl, about six years old, who died from an attack of smallpox, and I was assured that she had been vaccinated in infancy. If smallpox was in the immediate neighbourhood, it would be advisable to vaccinate all above eight or ten years of age.

Dr Pattison said he remembered smallpox breaking out in a house where there was a child that had not been vaccinated. I vaccinated the child, and it escaped. But a child in a neighbouring family, whose mother refused to allow vaccination to be performed, took smallpox and died.

Dr Keiller said that he understood that Dr Husband's opinion was that one vaccine vesicle was not sufficient, and that vaccination ought always to be performed on two places at least. He also held that there was much more fear of erythema in vaccinating adults than in children, and the scratches therefore ought to be very

slight. Dr Struthers, of Leith, was in the habit of vaccinating all his pregnant women.

Dr Young stated that he had not been in the habit of putting on more than one vesicle. There was often much erysipelatous inflammation, and he thought it was cruel to infants to vaccinate in more than one place. He always used tubes, and he thought it was of the greatest consequence that the tubes should be properly sealed. He had recently vaccinated with matter taken so long back as 1861, with perfect success. He did not believe in the necessity of re-vaccination.

Dr Gordon was of opinion that the fact of a person taking a second time, proved that he did not possess any immunity from smallpox.

Dr Cochrane said it was no uncommon thing to see cases of smallpox in persons who had been properly vaccinated.

Dr Taylor thought that we were bound to give to vaccination the same credit that Jenner sought for it. He said that vaccination would give the person as much immunity as an attack of smallpox would. He thought that the erythema so often met with in adults was owing to the movements of the arm. A good plan to prevent irritation is to place a piece of medicated cotton over the vesicles.

Dr Ritchie believed that arm-to-arm vaccination was best, but it is more convenient in practice to use tubes. He thought it was well to re-vaccinate in the event of smallpox being in the immediate neighbourhood, and that it is quite unnecessary to make any large scratch on the arm—better to puncture with the lancet dipped in lymph. He, however, thought it necessary to make two points by scratches in primary vaccination. The erythema in the adult, he thought, was caused by free living, state of system, or local irritation.

Dr Affleck said, that in vaccinating he avoided punctures, as they might produce pustules. He thought arm-to-arm vaccination the most successful. It almost never fails, but plenty of lymph should be put on.

Dr Bell said that when a medical man was asked to re-vaccinate, he could not see how he could well refuse. He always made two points, as in this way the likelihood of success was increased.

FŒTUS WITH THE MEMBRANES ENTIRE.

EXHIBITED BY ALEXANDER KEILLER, M.D.

MARCH 8, 1871.

DR KEILLER exhibited a fœtus with the membranes entire, which had been expelled prematurely. The patient had got cold and suffered from anasarca, and this had apparently been the cause of the abortion.

FŒTUS WITH THE MEMBRANES ENTIRE.

EXHIBITED BY G. STEVENSON SMITH, ESQ.

MARCH 8, 1871.

MR STEVENSON SMITH exhibited a similar specimen to above, which had been expelled entire, after much hæmorrhage, from a patient who had menstruated a month previously, and who did not know that she was pregnant. The pregnancy could only have existed for a few weeks.

MELANOTIC TUMOUR.

EXHIBITED BY A. M. T. RATTRAY, M.D.

MARCH 8, 1871.

DR RATTRAY exhibited a melanotic tumour, which had been removed from the right groin of a lady patient of his, seventy years of age, by Professor Lister, on 22d February 1870. A growth of a malignant nature afterwards appeared at the outer border of the cicatrix of the former tumour.

ON VACCINATION; WITH SOME CONSIDERATIONS REGARDING POSTPONED VACCINATION.

By R. Peel Ritchie, M.D.

March 8, 1871.

I had intended to submit the following considerations to the Society at the commencement of last session, about which time, it may be remembered, there was much agitation in London, and some parts of England, against the practice of vaccination. The number of communications, however, presented to the Society did not give me an opportunity to bring them forward. It seemed to me it was then advisable that some investigation should be made, as the opposition arose from other causes than ignorance or indifference, and believing, as I most firmly do, that the introduction of infant vaccination has been one of the greatest discoveries for the benefit of the human race, it appeared the more necessary that inquiry should be made,—1*st*, for the credit of vaccination itself; 2*d*, to allay popular disapprobation of, or disinclination to, its use; and, 3*d*, to support the good name of those engaged in its extension. To a professional audience in this place, a defence of vaccination is unnecessary; for, however greatly we may vary in our general ideas, I believe we are all agreed as to the protective benefit resulting from the introduction of the vaccine virus into the human system. I do not, therefore, propose to offer a defence of vaccination, considering that there are no dissentients in this Society as to the advisability of infant vaccination; nor need I occupy your time by combating the view of those who allege that smallpox is now less virulent, because, following a recognised pathological dogma, it, common with other epidemic diseases, has, in progress of years, become of less severity. Meantime, I content myself, on the authority of those able to express an opinion, with a simple denial of the fact that smallpox, in the

unvaccinated, is now a milder disease than in former years; and by the correction of the statement of the pathological law alluded to, which has reference to the virulence of an epidemic becoming less in proportion to the duration of an individual epidemic, and not as to the virulence of the disease when it acts on a community at another time. Were this doctrine to be held always true, we would not, in Edinburgh, so frequently be mourning the loss of so many little ones from hooping-cough, nor would our mortality returns have shown so many deaths from scarlatina and measles. An individual epidemic does apparently in its course wear itself out; but I cannot allow that epidemic diseases invariably follow the law that, from their repetition, they become milder. I may point to the recent severity of smallpox in Paris, London, and Liverpool, in confirmation of my statement. The opponents of vaccination—who, I am glad to say, are, demonstratively at least, few in Scotland—chiefly raise their objections on the grounds that vaccination is not protective from smallpox, and that its practice is calculated to have injurious effects on the infant operated on. In reply to the first of these grounds, I consider that the statement issued by the Registrar-General for Scotland, in his Fourteenth Report, that for 1868, speaks for itself—"That never since the prophylactic powers of the vaccine virus extended to Scotland in 1800 have so few deaths from smallpox occurred there." From 1855 to 1864, the deaths from smallpox constituted 1·7 per cent. of the total mortality in Scotland. "Since that period the Vaccination Act has been in full operation, and for five years—1865 to 1869—its beneficial effects have been manifested in this, that during these years only 2-10ths of a percentage of the deaths have been caused by smallpox." These extracts give us, I think, a very good argument as to the protective benefits of vaccination. The other ground is, that vaccination is injurious to the child. So far as the prejudice against it arises from injury to the *life* of the child, the Registrar-General further states that, "in 1868, there

were vaccinated in Scotland 106,181 children, and of that number two were recorded to have died from the consequences thereof; that is, one death in 53,090 cases." He continues—"Now, the constitutions of some children are such that the slightest scratch or abrasion of the skin is followed by inflammation and death. Need it be wondered at that two deaths should occur from that cause among the 106,181 children whose arms had been scratched to insert the vaccine virus? The astonishment is, not that there were deaths, but that the deaths were so few."

There only remains the other part of the second ground of objection to be considered, that vaccination is injurious to the *health* of the child. The passage I have quoted from the Registrar-General is just one of those which is likely to give the opponents of vaccination a standing-point. If, they may be supposed to say, certain children are of the temperament or constitution described—that a scratch or abrasion is followed by inflammation or death—why subject a child to this risk, on the plea that if they had been innoculated with smallpox many hundreds would have died, and that if the vaccination had succeeded, the child's life would have been better protected against smallpox? This line of argument seems to me more likely to increase the opposition than to allay the prejudice; and what I propose to do in what follows, is to offer to the Society some considerations which suggested themselves whilst I held the office of Extra Physician to the Children's Hospital; and in doing so, I may perhaps be able to turn the practice of that institution to general account.

To carry out the object I have in view, I propose, therefore, to consider—

1. The age at which vaccination should be performed.
2. In what circumstances or instances it should be delayed.
3. In what circumstances or instances it should be postponed beyond the time permitted by the Scotch Act. And,

4. To offer for your approval or disapproval certain deductions which appear to me to be suggested by these considerations.

1. *The Age at which Vaccination should be Performed.*—On this point there appears to be some variety of opinion; for whilst in one part of the United Kingdom the operation must, according to law, be performed within three months, and the most suitable time is considered to be at six weeks, in this portion of the kingdom a more extended period is allowed. That vaccination may be successfully performed a few hours after birth, the records of our Society show; and the opinion of various vaccinators is to the effect that the operation is successfully performed, and without varying results, at all ages. Thus, Ballard * observes that Husson, in 1821, stated that, "After twenty years' experience, during which he had vaccinated at all ages, from immediately after birth to extreme old age, he had never been able to observe any remarkable difference in the results of the operation" (p. 186); and again, the the same author, quoting from the French report upon vaccination in 1860, states that, "During six years, it had been customary to vaccinate every week several newly-born children from the Clinique d'Accouchement of the Faculty, and that the reporters had constantly observed that everything proceeded in such infants just as it did in children more advanced in age" (p. 187). Argument on the capability of being vaccinated at a very early age is not, however, likely to arise; for if our personal experience should not furnish us with evidence, there is abundant recorded evidence of the success of the vaccination. The question therefore I would propose is, Is it expedient? To this query my reply would be, that unless when smallpox was threatening or manifested in a district, it is *not expedient*. Smallpox being absent, I consider the following as some of the reasons why early vaccination is not expedient:—

* On Vaccination: its Value and alleged Dangers. A Prize Essay. By Edward Ballard, M.D. London, 1868.

1*st.* That, to weakly babes, injury may be done by the constitutional effects of the introduction of the virus into the system.

2*d.* That injury may be done by the local irritation inducing convulsions, erysipelas, etc.

3*d.* That at birth a constitutional and hereditary tendency to disease may be latent, which, developed by the vaccine febrile state, would, without doubt, be credited to the operator or his operation.

4*th.* That, although the French observers are of opinion that very young infants are not less liable than older children to take smallpox, an opinion is held by some in this country, that very young infants are less liable to take smallpox, and that the protective effects of vaccination are therefore less called for in them than in infants some weeks old.

5*th.* As syphilis, when not observed at birth, does not generally show itself by its peculiar eruption till the sixth week of life, and sometimes not until the twelfth, it would appear to be more prudent that the operation be delayed till after the third month.

6*th.* Infants who, previous to vaccination, have suffered from syphilis and have apparently recovered, often again exhibit the eruption after vaccination; and infants who have suffered from the milder indications of syphilis, but in whom the eruption has been slight and transient, and who therefore have not been subjected to special medication, will frequently become the subjects of syphilitic eruption subsequent to vaccination.

7*th.* The general success of compulsory vaccination in Scotland, as opposed to England, appears to be also in favour of my argument.

From the consideration of these propositions, the results of views formed in the Dispensary of the Children's Hospital and in private practice, I have come to the conclusion that vaccination should only be performed antecedent to the third month in exceptional cases. This is a view antagonistic to the instructions of the

National Vaccine Establishment and English authorities, who "recommend six weeks as the best age, except when children are delicate or suffering from disease" (Seaton's "Handbook of Vaccination," p. 102). Between the completion of the third and the completion of the fourth months, is, I consider, the most appropriate time for the performance of the operation. After the end of the fourth month, I include those cases forming the subject of my second question, to the consideration of which I proceed, viz. :—

2. *In what Circumstances or Instances Vaccination should be Delayed.*—It should always be held in mind that the practice of vaccination in this country is universal, and that the law is directed to the rich as well as to the poor. We must not, therefore, in deciding on the subject, draw our conclusions from one class rather than the other. Whatever the position in life may be, the appearance of injury following a compulsory act is likely to be indignantly resented; and whilst, to the private practitioner, any accidental occurrence in, or unusual results from, vaccination may be calmly explained to an intelligent parent, in the public prosecution of the operation—which, amongst the not so intelligent poor, is too often only regarded in the light of an inconvenience to themselves and an annoyance to their children, to which the law compels submission, under threat of a penalty, or even imprisonment—there is still greater need that the nature of the operation, the necessity for its performance, and the accidents which may follow it, be carefully and explicitly explained. It is amongst those seeking the public or gratuitous performance of the act that accidents following it may be expected. In private, the family practitioner is not likely hastily to vaccinate a child regarding whose state of health he entertains doubts. But, in public vaccination, when the number of applicants is large, there may neither be the opportunity nor time to make a minute personal examination of the child, and the mother's statement, given honestly enough, but often

incorrectly, that the child is healthy, may be taken as sufficient to warrant our proceeding with the operation. A surgeon, before proceeding with an operation of a more extensive nature, but perhaps less serious constitutional results, would not be satisfied with the patient's statement alone; and I contend, therefore, that before a child, seen for the first time, is vaccinated, bearing in mind the statement I read from the Registrar-General of Scotland's Report, a thorough examination ought to be made, so as to satisfy the operator that, so far as he can judge, the operation may be safely performed, and that, too, not only with safety to the patient, but with credit to the practitioner and to the operation.

Vaccination is so easily effected, and is so frequently performed, that its character as an operation is almost ignored; and whilst a poisoned scratch in the post-mortem theatre or the dissecting-room would be most carefully looked to, the virus of vaccination is introduced into the systems of children by tens in the course of a few minutes, and from the frequent repetition of the act it comes to be regarded as a mere trifle. Now, an act which exercises such undoubted influence over the system of an individual for seven or more years—and in my own person for upwards of twenty-five years—cannot be regarded as a simple matter or a trifling operation. It is not one to be performed without consideration; and it seems to me that vaccine stations, although excellent in theory, so far as enabling a large number of children of the poor to be readily vaccinated at one time, have objections, arising from the large number seeking attention there, and it may even be preventing the operator making that careful examination of each individual which, in my opinion, should precede the operation.

In what circumstances or instances, then, should vaccination be delayed beyond the fourth month?

As a general answer, I should say, in every instance in which the child was not in good health.

In the practice of public vaccination the strict adherence

to this rule may be attended with many inconveniences to the parent, for, as has sometimes happened in this city, mothers have walked twelve, seven, and six miles, and home again, to have their children gratuitously vaccinated at the Royal Dispensary and Vaccine Institution.* It may be necessary, therefore, in certain exceptional cases, for the sake of the parent, to depart from the strict carrying out of this rule. In the following instances, both for the credit of the operation and the good name of the operator, I hold no departure from the rule should be made:—

(*a.*) When the febrile state is present.

(*b.*) When there is evidence of any local inflammation.

(*c.*) When bronchitis is present.

(*d.*) When pertussis exists.

(*e.*) When there is diarrhœa.

(*f.*) If struma is manifesting itself by local inflammations, abscesses, or markedly impaired nutrition.

(*g.*) If the child has night-crying and hoarse, snoring, or snuffling respiration, whether or not there has been other previous evidence of specific infection, such as eruption, condyloma, mucous patches of mouth, or scaly erythema of nates.

(*h.*) If scabies, impetigo, or eczema, are present.

(*a.*) The febrile state.—As a general rule, from the time of Jenner the existence of the febrile condition has been regarded as an obvious reason for delaying vaccination. Seeing the child for the first time there may be uncertainty as to what the condition may result in, and, as the illness is not chronic, no harm is likely to ensue from a temporary delay. On the other hand, the introduction of the vaccine virus is, it may be said, certain to produce exacerbation of the morbid condition. It will be proper, therefore, on account of the child, to delay till the febrile reaction has subsided.

(*b.*) A child suffering from a local inflammation is

* See Annual Report for 1864.

sufficiently unhappy, without having an additional cause of discomfort introduced—a cause, be it remembered, not producing merely a local irritation, but a general action; and one, too, which even in health is accompanied by febrile reaction.

(*c.*) I have seen children, with the vaccine vesicle running its course, suffering from severe bronchitis. It is not a pleasant sight. It is a coincidence which should not occur, and it has appeared that the sufferings due to the bronchial inflammation were very greatly aggravated by the discomfort caused locally and generally by the vaccination.

(*d.*) Pertussis.—Among the varied cures for this disease, vaccination has, I believe, been suggested: the circumstances in which it is likely to be beneficial must be limited, for the two diseases co-existing cannot but render an unhappy child still more so. Independently of the frequent association of the disease, considered under the last head, with the pertussis of young infants, and in the course of which, as shown, I hold vaccination to be at least injudicious, pertussis presents objections to the operation. Amongst others I may indicate the risk of local aggravation of the vesicles by rubbing, caused by the arm being thrown about when the paroxysm supervenes, also, the risk there always is in pertussis of nervous or other complications arising, the intractable nature of these, and the danger there may be of having to contend with three distinct morbid conditions in the person of an infant. The delayed formation of the areola has been noted by observers in pertussis. Dr Seaton states, "One would never hesitate to vaccinate a child whose general condition was fair, merely because it was going through the chronic stage of hooping-cough" (p. 100). With all deference to this authority, I certainly would hesitate, unless the near presence of smallpox rendered the operation immediately expedient.

(*e.*) Diarrhœa.—Although this malady in infants is frequently functional in its character, still it may be

rapidly fatal. In constitutionally sound children I have not observed its occurrence to a severe extent along with vaccination, but in strumous children I have repeatedly seen it and vaccination coincident. Although there was no direct connexion between them, I am nevertheless of opinion that whilst there may be risk of the operation being futile whilst diarrhœa continues, it is inexpedient to have vaccinia running its course whilst the infant is suffering from so reducing an ailment.

(*f.*) Imperfectly nourished strumous children are frequently presented for vaccination. The duration of their life appears to be limited; and in practice I have been in the habit of delaying vaccination, and recommending such general treatment as appeared to be suitable so as to give the infant a chance of improving in its vital condition before the first six months of its wretched existence had passed. Too often, I fear, the grave has received them; but I form this opinion from the fact that they did not return for vaccination, not from certainty of their death. They have in most instances been the progeny of our nomad population—of those who have no fixed residence, and labour at no fixed employment. But I can recall instances from our own resident population—underfed and miserable, from causes which too readily occur to the minds of those familiar with the state of the lowest class of our inhabitants, just for the time above, but rapidly approaching to, the position of paupers. No vaccinator, even in the less aggravated conditions of struma, would, I suppose, consider the vaccination of such infants either advisable or prudent, unless an epidemic was imminent, or to act as a prevention to others.

(*g.*) Syphilis.—Children suffering from syphilis were frequently brought to the Children's Hospital for vaccination, and I have repeatedly declined vaccinating them. It is, perhaps, when this cause for delaying the operation is met with, that the wise provision of the Scotch law, allowing six months rather than three before penal steps can be taken by the authorities, is evident. The greater

length of time of our law had not, however, its origin in any consideration regarding the state of health of the child to be vaccinated, but arose from the difficulty in having children vaccinated within three months in all parts of the country. This provision of the law I regard as very beneficial, and in this diseased condition is of the greatest importance. In my experience no constitutional state gives rise to more frequent complaints as to the injury done by vaccination, and no constitutional state has less cause for being blamed. In this city there is frequent opportunity to study the phenomena of this constitutional disease. The conclusion I have come to is, that syphilis, when fatal, generally proves so within the first six months of life. If the unfortunate victim of it survive that age, the probability is that complete recovery from it will subsequently take place; as a rule, therefore, I rarely vaccinate a syphilitic child until the first six months have passed; and this rule I follow, even when the child has been subjected to appropriate treatment, and apparently presents no indication of the disease. Fortunately, the signs of syphilis are generally decided; they commence, if not pronounced at birth, about the third week. About the third month, if the case is mild, and even although no constitutional treatment may have been adopted, the cutaneous eruption frequently, or even very generally, disappears, but only to return when the health of the child is deteriorated by any cause. Vaccination, establishing a local irritation and a general reaction, will frequently reproduce the characteristic eruption; and, by a mother, on the one hand, unobservant and personally unconscious of her own contaminated state, and on the other sometimes, as I have found, wilfully ignoring it, the appearance of a rash subsequent to the child being vaccinated, is likely to be attributed to the injurious effect of the operation. In a recognised case, I always warn the mother, even although her child at the time appears healthy, that possibly an eruption will appear. But in cases submitted to the operator for the first time,

I am not satisfied with the mere freedom from eruption. Two symptoms, when co-existing in a child, always seem to me to point to this peculiar constitutional state, and when found combined it is advisable to follow up their suggestion by further inquiry; these are night-crying, and hoarse voice, with or without snuffling respiration, however slight. As illustrating my meaning, the following case may be quoted:—An infant was brought to me at the Hospital. The mother stated she had been to various doctors, but they did not appear to be aware of what ailed her child. Hearing the child cry, I marked the roughened note, and in answer to my query, I found that night-crying had been a marked peculiarity from birth. Following up the hint, I charged the mother with having had miscarriages; she owned to three, the infant she had then in her arms being the result of the fourth pregnancy. Her case was peculiar; she had three healthy children, then three miscarriages, and lastly the present child; she denied having had any genital venereal affection; and being an apparently respectable married woman, I pushed my inquiry no further, but treated the child according to my diagnosis; the child subsequently, but not unexpectedly, died. I was then introduced to the father, who requested a certificate of the cause of the child's death. My inclination was to charge him with having given his wife disease; but, approaching the subject more guardedly at first, he was questioned as to his morality; he denied my imputations, and I felt satisfied from his manner, that if his wife had been diseased, as I firmly believed, she had not contracted her malady from him. I filled up the certificate, and had handed it to him, when, just as he was leaving my room, he turned, and said that perhaps he could throw some light on the matter, and the following was the explanation he gave, which to me is satisfactory enough, and confirmatory of my diagnosis:—His wife was the daughter of a soldier. Before her birth her mother was diseased; her father died; and after his death her mother returned to Portsmouth,

where she led an abandoned life. Almost in a dying state, she had opened communication with her daughter, who received her into her own house. She was then diseased, and was waited on by her daughter, who, in dressing her sores, became herself affected; and delicacy of feeling had apparently prevented her offering the explanation at first. It was evident that the appearance of constitutional disease in the grandchild had not been due to a hereditary state of the mother, for in that case the miscarriages would have been in the first pregnancies; and I found that the date of the first miscarriage was subsequent to the grandmother taking up her residence with her daughter. Innoculation of the venereal virus, it was stated, had taken place through the hand. My diagnosis in this case was founded on night-crying, and the hoarse vocal note.

The three following causes of delay have, since the days of Jenner, been regarded as contraindicating vaccination. Thus, in his "Letter on the Influence of Artificial Eruptions" (Lond. 1822, p. 32), Jenner remarks, "What I have repeatedly said elsewhere, respecting our sweeping all eruptions from the skin previous to, or at the time of, our inserting the vaccine lymph."

(*h.*) Scabies sometimes complicates vaccination, and a very slight examination may enable this to be recognised; it is self-evident. The combination of the two is very troublesome to the child, to the nurse, and to the medical attendant.

Impetigo.—When impetigo exists, the vaccination should always be delayed. If any irritation is made on the skin when this disease is latent, it is almost certain, before long, to make itself manifest. How frequently do we notice the impetiginous eruption appearing in the neighbourhood of a wound or scratch! When impetigo exists, or has recently existed, the vaccination should be delayed.

Eczema.—For two reasons I do not vaccinate when eczema exists or has recently existed. Like the foregoing,

it is connected with the state of health, and when vaccination is performed during its existence, there is uncertainty as to the result. It frequently shows itself when the integrity of the integument is interfered with; it is also in children very often associated with syphilis. There is uncertainty as to the success of the vaccination, or there may be uncertainty as to its too great activity. As some further observations on the complications of vaccinia by eczema will be made under the third division of my subject, I shall not further dwell on it now.

In these ten instances—and their number might be increased—I am of opinion that vaccination should be delayed. In private practice, the Fellows of this Society would most likely decline vaccinating where any of these complications existed; and if, in the public prosecution of the operation, attention was always given to them, it occurs to me we would hear much less of the opposition to vaccination which, previous to this epidemic in England, furnished certain obstructive persons with the means of obtaining an unenviable notoriety.

I proceed, therefore, to the third division I proposed: *In what circumstances or instances should vaccination be postponed beyond the time permitted by the Scotch Act?*—My experience in the Dispensary department of the Children's Hospital convinced me that to postpone vaccination amongst the children who were presented there for vaccination was frequently necessary. The necessity for it occurred, not only amongst the children who had been previously attending for medical treatment, but also amongst those presented for the first time. The records I have are not perfect. Certificates of postponement were frequently given, and I regret to find, in a considerable number of cases, no record of them in the duplicate form has been kept; and even in the cases recorded, in three of them the cause of the postponement is not stated. My attention was not so much directed to this matter at first, and my personal supervision was not therefore given to the registering of the diseases. I am not, consequently,

in a position now to state the proportion between the cases postponed beyond six months, and those who applied for and were vaccinated. The Dispensary department of that Hospital is a favourable place for considering the necessity for the postponement of vaccination, and presents opportunities such as a vaccine station does not perhaps do, for investigating this question, as well as observing the effects of vaccination on diseased children.

I propose, therefore, to consider, 1*st*, For what causes vaccination was postponed; 2*d*, For what time the postponement was necessary.

1*st*. *For what causes vaccination was postponed.*—I have the names of 25 children who, having attained the age of six months, were not, in my opinion, in a fit state of health to be vaccinated. In three of these the child's name and duration of the postponement are given, but the cause is not. As the names of these children do not appear on the General Register of Diseases at the Hospital, it may be concluded that they were children brought to the institution for the first time, and, being found diseased, the operation was postponed.

The number of instances is consequently reduced to 22. An analysis of these cases gives the following result:—

2	were	postponed	on account of convulsions.
2	„	„	chronic meningitis.
5	„	„	eczema (1), of scalp (3), of face (1)
3	„	„	syphilis.
1	„	„	„ with pertussis.
1	„	„	syphilitic lepra.
1	„	„	pertussis.
1	„	„	bronchitis.
2	„	„	struma.
1	„	„	varicella.
1	„	„	improper feeding.
1	„	„	temporary insusceptibility.
1	„	„	scabies.
22			

Of the causes, eczema takes the lead in nearly one-fourth of the cases. Of the propriety of postponing vaccination in this disease—a disease I regard as one of the most obstinate as regards treatment with which young children are affected—I have now no doubt.

The introduction of vaccine virus into the system of a child inclined to eczematous eruption is peculiarly unsuitable. I am aware, in this disease, authorities state that whether vaccination will take effect is doubtful; my experience would rather lead me to ask, when vaccination is properly performed in this state of system, whether the effect is not likely to be too severe? I ask this question, both from the results obtained in some of these five cases, and also from repeatedly having children brought to me suffering very severely from eruption subsequent to vaccination. With the exception of impetigo, syphilis, and perhaps erysipelas, I know no disease which is calculated to bring vaccination into more disfavour than eczema, and there is no state of system, with the exception of the two first named, which gives me more solicitude when the time for vaccination approaches. I now invariably advise that the operation before the fourth month be *delayed*, and, if necessary, afterwards *postponed*. The state of system in which this disease occurs is peculiarly deceptive. By treatment local and general, you may, perhaps, congratulate yourself that the eruption is at last subdued; that, although there is a mere trace of it about the ears, as the statutory period for vaccinating the child has nearly expired, the operation may be safely performed. But the manifestation of the disease is only latent, and it but requires the vaccine irritation to re-excite it, and you soon again have the eruption spreading over face, head, and arm. I have seen, however, that in subdued chronic eczema, re-excited by vaccination, subsequent freedom from the disease was obtained. This I do not regard as the general result. In one of the cases the effect of vaccination was singular; so much so, that its narration may be excused:—

James G., before he was three months old, showed the indications of eczema. He was a stout child, of rachitic conformation. The eruption was chiefly confined to the head and face, but also extended subsequently to arms and trunk. At six months, although suffering in a much less degree, I considered it advisable to postpone vaccination for two months, and certified accordingly on 10th February. In April I certified again, and delayed operation till June. He was now so well that I considered I might vaccinate without bad result; the operation was done on 10th June, the child being now ten months old. On the 15th two spots exactly like the vaccine vesicles, with inflammatory zone, showed themselves on the vaccinated arm (in addition to the spot operated on) in the neighbourhood of previous eczematous patches. On the 17th, they had the appearance of seventh day vaccinia. On the 19th, there were about seven distinct supernumerary vesicles at wrist, and also at ankle. Those on the arm had got broken, and the arm was inflamed up to the axilla. The eruption at wrist and ankle was attended by a good deal of febrile reaction. In this case the secondary eruption was apparently the result of absorption, and these vesicles left cicatrices corresponding to the mark of vaccination.

My own opinion as to the advisability of delay in this disease is decided—a great risk is run in causing an unmanageable form of eczema; and also from the foregoing it would appear that in this diseased state the constitutional effects of vaccination may be occasionally too violent.

The disease for which vaccination was postponed in next frequency was syphilis. In one instance it was complicated with pertussis. In another the disease was limited to the eruption of lepra; and in three the ordinary constitutional indications of the disease occurred. Successful vaccination may be performed in this disease, but it appears to me there can be no question as to the advisability of postponement, on account of popular objections arising from the consequences. As alluded

to it under the second division of this paper, it is unnecessary now to say much more. When there is even the supposition of its being present, I consider it prudent to postpone the operation; and I found this opinion, 1*st*, on the injury likely to be done to the patient by the exacerbation of the constitutional taint before the appropriate treatment has been adopted; and, 2*d*, from the bad name the operation sometimes receives, in consequence of the latent disease being manifested by it. This bad repute tells in two ways: first, that the operation itself is injurious; and second, that lymph, although known to be free from taint, is often charged with being the means of conveying the constitutional disease. Whilst I do not deny, but I think it very improbable, that syphilis may be communicated in vaccine lymph, I do hold that, so far as I have seen, a latent constitutional taint becomes rapidly developed, and speedily indicates itself by the manifestation of the usual *chronic* phenomena of constitutional syphilis after vaccination. But whilst not denying that the disease may be communicated, I think it worthy of remark, that in not one of the many cases brought to me at the Children's Hospital or Royal Dispensary was there evidence of a syphilitic primary sore on the arm; the vaccine scab was there, and the eruption of syphilis was visible on the body, but no chancre could be found. It is singular, if the disease were, as stated, innoculated, that the manifestation of its having been so was in the secondary form!

This cause of delay or postponement occurs not only amongst the infants frequenting our charitable institutions. One of these cases occurred in the family of a private patient. In the case complicated with pertussis, there was a complex reason for delay; either it or the constitutional taint was sufficient reason. Pertussis, as a cause of delay, was already alluded to. The combination of vaccination and pertussis may be injurious. I have observed convulsions to complicate such a case, and I observed purpura in one chronic case in 1866.

In four instances the postponement was due to nervous disease. Two of these were convulsions. One of these cases was of an obstinate but interesting character, and at last vaccination was performed without any bad result. In the two other nervous cases, chronic meningitis was the cause. One case died soon after certificate of postponement was given. The other was in very feeble general health, and, in consequence of my retiring from the Extra Physicianship at the Hospital, I am unable to trace the subsequent history of the child. In chronic head affections of children, slight causes will often determine the result. The supervention of any fever in these cases I regard as of the gravest importance, and as vaccination is calculated to produce the febrile state, it should not be proceeded with.

Bronchitis being acute rendered postponement necessary in one instance; comment upon this is superfluous.

Varicella caused delay in one instance; being an acute disease, postponement or delay in consequence is only following the general rule. Besides, I am inclined to believe that the presence of varicella has a modifying power on vaccinia, and that the vesicles are not perfectly formed in consequence. I make this observation, however, upon limited experience. That it may delay the formation of the vaccine areola has been recorded by observers.

Scabies postponed in one instance.

Improper feeding is stated as a cause of delay in one instance. This cause requires some explanation. It means that the general health of the child was so bad in consequence of proper diet not having been given to it, that it was in too low a state of system to undergo the operation. Comment is uncalled for.

In two instances struma postponed the operation. As a constitutional disease, if acute symptoms are present, caution should be exercised as to vaccination, for it is highly probable that in these cases glandular enlargement or abscess will arise, and amongst the ignorant the vaccine virus may often get the blame or discredit.

The last cause of postponement was, that after two properly conducted attempts to vaccinate, the result was nugatory. I have never met with a case which could be called insusceptible, but I have met with similar instances to the one under consideration. Failure at the first attempt I find to be rare; and when it has occurred, I have been inclined to blame those in charge of the infant for allowing the virus to be rubbed off. When I know or fear this is likely to occur, a little more care to introduce the point of the lancet charged with lymph below the cuticle will obviate this cause of want of success. But in this case, after two attempts, I came to the conclusion that for some reason there was temporary insusceptibility. The case occurred in summer. In reference to this, I note that Dr J. C. Dickenson, recently referring to what is already on record, states "that in India vaccination is only performed during the cold season, and that during the hot and rainy seasons all attempts have failed; and suggests that vaccination in England during the summer months may also be influenced, and that, if cases were carefully noted, the present view, that vaccination is equally successful and protective, no matter at what season of the year it is performed, might be modified." Now, in my case, I do not think this was the explanation; for, at the same time, and with matter from the same source, I succeeded with others. My idea is, that the system is not always equally receptive of the virus, and that, instead of repeatedly attempting to vaccinate, it is better, after a second failure, to delay for some weeks or months, and then to renew the attempt. In this way I have found that I have as yet avoided the necessity of granting a certificate of insusceptibility.

I proceed next to consider *for what time the postponement was necessary.*

Of the twenty-five cases, postponement was only necessary for two months in seventeen cases; amongst these were included the following causes:—Bronchitis, convulsions, chronic meningitis, eczema, pertussis, varicella,

scabies, specific lepra, syphilis (2), struma (2), improper feeding, and temporary insusceptibility. The three cases whose disease was not recorded are also included in this number. These seventeen cases may be taken as representing the acute causes of postponement.

In two instances, delay for four months was necessary on account of eczema.

In three, postponement was occasioned for six months; in two, the cause was eczema; in one, pertussis with syphilis.

In one case, delay for eight months occurred; the cause was syphilis.

In one case, chronic meningitis caused postponement for ten months.

And lastly, in one case, convulsions occasioned delay for fourteen months.

Those cases bear out my previous remarks. Two of the nervous cases caused the longest delay. Syphilis (in one instance complicated with pertussis) was not cured till the children attained the ages of fourteen and twelve months, and eczema, showing its chronic persistent character, prevented the children from being vaccinated till they had attained the ages of twelve and ten months.

The intention of this communication being to place before the Society some considerations calculated to lessen the objections to compulsory vaccination, the following deductions seem to be warranted by what has been stated:—Vaccination is to be considered from two points of view,—1*st*, the benefit of the community at large; and, 2*d*, as objected to by certain members of that community. Now, the State has taken upon it to decide that the community is to be protected, and that the objectors must submit to a supposed personal injury in order that the community may be protected. Taking the same view of the question as the State does—the advisability of, and the necessity for, this protection—I would insist on universal vaccination of all infants born within these realms when they are in a fit state of

health to be vaccinated; but, on behalf of the objectors, many of whom, if not all, I cannot but consider as conscientious objectors, I venture to put before you the following deductions, previously observing that the general success of the compulsory vaccination has apparently been greater in Scotland than in England, and that the demonstration against vaccination has met with little encouragement amongst our intelligent inhabitants:—

1*st.* Viewing the question from the State position, there is no doubt, if a nation is to be protected from an invasion of smallpox, that the *earlier* vaccination is performed the better for the community.

2*d.* Viewing the question from the objectors' position, there is no doubt the *longer* vaccination is delayed the better.

3*d.* There is abundant evidence that vaccination may be successfully performed at the sixth week of life.

4*th.* Vaccination performed at the sixth week on a child, seen for the first time, may be performed on a child suffering from latent constitutional or other disease, and so bring the operation and the operator into disrepute.

5*th.* From the objectors' position, vaccination performed after the third month is less likely to give rise to agitation, because by that time serious constitutional disease is more likely to have made itself manifest, and therefore the operation is less likely to bring the practice into disrepute or to cause reflections on the operator.

6*th.* Vaccination may be performed on an infant, either at six weeks or three months, who is not in perfect health, without injury to life, but with a risk of the development of disease, which may be troublesome.

7*th.* Although vaccination may be performed at these ages without injury to life, still any external disease arising subsequently is not unlikely to be attributed to the effects of vaccination by those opposed to it.

8*th.* For the credit of vaccination, to prevent or allay popular disapprobation, and to support the good name of the operator, vaccination, except when smallpox is

threatening, should never be performed at any age until the vaccinator is assured that the infant to be vaccinated is in good health.

9*th.* Judging, both from the State position and that of the objector, from the success of the compulsory Act in Scotland, six months ought to be allowed before any compulsory power be enforced.

10*th.* That, for the good of a community, compulsory vaccination is necessary.

11*th.* In order that objections to compulsory vaccination be lessened, there should be power to grant certificates of postponement beyond six months when considered necessary.

12*th.* That such certificates should state the reason for the postponement, and should be paid for by the State.

13*th.* That all certificates connected with primary vaccination, and in certain cases the operation itself, should be paid for by the State. This would not prevent private patients giving an additional fee for the operation suited to their position.

14*th.* To lessen the amount of work to be performed by one operator, and to prevent too many candidates for vaccination applying at the same time, on behalf of the objectors, any qualified medical man should have the power or privilege of performing and of certifying to vaccination, and that such certificate should entitle him to be remunerated by the State.

15*th.* That certificates of successful vaccination should only be given, and stated to be "on personal investigation."

16*th.* That a Registrar, as in Scotland, should not only have the power to enforce the registration of all children born in his district, but should also have the power to enforce that a certificate of successful, postponed, or insusceptible vaccination be presented to him within a statutory time.

And I would also beg leave to suggest,

17*th.* That, on new families arriving in any town or

district, the Registrar or other officer should have authority to ascertain if the children have been vaccinated, and to enforce the law if necessary. This supervision might be placed under the control of the police.

Dr Cochrane thought that Dr Ritchie's remarks regarding the remuneration of medical men for certificates were most appropriate.

Dr Pattison said he had used lymph from patients who had been re-vaccinated with success.

Dr Coghill thought that the question was so important, that a committee should be appointed to draw up some resolutions for presentation to the Secretary of State.

Dr Thomson seconded the proposal; and it was agreed to refer the matter to the Council, with the addition of the following gentlemen:—Drs Coghill, Bruce, Simpson, and Husband.

PLACENTA SHOWING CLOTS AND INDENTATIONS ON ITS SURFACE.

EXHIBITED BY PROFESSOR SIMPSON.

MARCH 22, 1871.

PROFESSOR SIMPSON exhibited a placenta showing clots and indentations on its surface. The case was one of accidental hæmorrhage; the child was about the seventh month, and was born dead.

THE MECHANISM OF THE EXPULSION OF THE PLACENTA.

BY J. MATTHEWS DUNCAN, M.D.

MARCH 22, 1871.

IN this short paper, I use the comprehensive word "mechanism" in the limited sense and misapplied

manner in which it has hitherto been generally employed in obstetric literature; that is, as implying mere relative position at different stages of progress.

The present subject has not, I believe, attracted sufficient attention, else I am sure the little addition to its history that I propose to make would, long ere this time, have been contributed. No one will dare to say it is unimportant; for there is no truth in nature, which, however insignificant it may appear, has not even now, or may not have in future, bearings upon practical rules which may be of value to the obstetrician.

A study of our copious obstetric literature satisfies me that the point I propose to describe in the mechanism of labour is generally misunderstood. To illustrate the erroneousness of widely-entertained and generally-written views, I might refer to many authors, and to woodcuts and plates. Among the former, I select only the excellent and respectable Ramsbotham, who says,* "The placenta passes through the vagina inverted, so that its fœtal surface becomes external." Among the latter, I select the most recent, and the most elaborate and largest—one of the wall-plates † of Professor Schultze of Jena, from which the plates 1 and 2 are copied on a reduced scale as to size.

Every one knows that the membranes are expelled inverted or flapped over upon themselves; and the same view is held regarding the placenta. It is to this last part's condition during expulsion that I am now directing attention.

The wall-plate of Schultze is an admirable representation of the expulsion of the placenta as it occurs in the first two modes described by Baudelocque; and I may add that this author's descriptions have been repeatedly copied. Baudelocque, drawing chiefly on his imagination

* Obstetric Medicine and Surgery, 5th edition, p. 126.

† Wandtafeln zur Schwangerschafts- und Geburtskunde.—An excellent and useful work.

N° 1.

SEPARATION & EXPULSION OF PLACENTA,

after Schultze.

EXPULSION OF PLACENTA,
after Schultze.

for his supposed facts, says:*—"Sometimes this separation begins at the centre of the placenta, and sometimes at some point of the circumference, which produces different phenomena. In the former case, the middle of the placenta being pushed forward, it forms a bag behind which fills with blood, and it presents that side to the touch which is covered with the membranes and vessels. The placenta forms nearly a similar bag, and presents in the same manner, when it begins to separate from the uterus at that part of its edge which is furthest from the orifice. But things go on very differently when the separation begins at its lower part, especially if it be in the neighbourhood of the orifice. In this latter case the placenta rolls itself up in the form of a cylinder, and according to the length of the uterus, so as to present its anfractuous surface to the touch; and its exit is always preceded by a little fluid blood." Schultzet goes a little farther than Baudelocque, and actually describes the accumulated hæmorrhage from the uterine sinuses as co-operating to push down the already almost completely detached placenta and complete its separation,—a view so utterly unsupported by observation or argument, and so unlikely, that I shall not say anything more regarding it. (*See* Plates 1 and 2).

Now, the erroneous belief that the placenta generally

* System of Midwifery, Heath's translation, vol. ii. p. 4.

† Wandtafeln. Tafel xvi. fig. 3, Demonstration der Wehen. Schultze's words are:—"Schon die Wehen, welche das Kind vollends austrieben, verkleinerten die Gebärmutter so bedeutend, dass der Mutterkuchen zum grossen Theil von ihrer Wand abgetrennt wurde. Die Nachwehen und der Bluterguss, der aus den zerrissenen Blutadern der Gebärmutterwand reichlich erfolgt, drängen den Mutterkuchen vollständig von der Gebärmutterwand ab und in die jetzt lehre Eihöhle hinein." A similar mechanism is described by Spaeth. Speaking of the pains, he says:—"Unter ihrer Einwirkung wird der Uterus allmählig kleiner, seine Innenfläche zieht sich über die entsprechende Fläche der Placenta zusammen und vollendet die Lostrennung der letzteren, welche schon in der vorigen Geburtsperiods begonnen und das abfliessende Nachwasser blutig gefärbt hat, gewöhnlich derart, dass die Placenta zuerst in ihrer Mitte gelöst mit der Fötalfläche durch den Muttermund heraus gewölbt und endlich vollends in die Scheide herunter getrieben wird."—*Compendium der Geburtskunde*, s. 85.

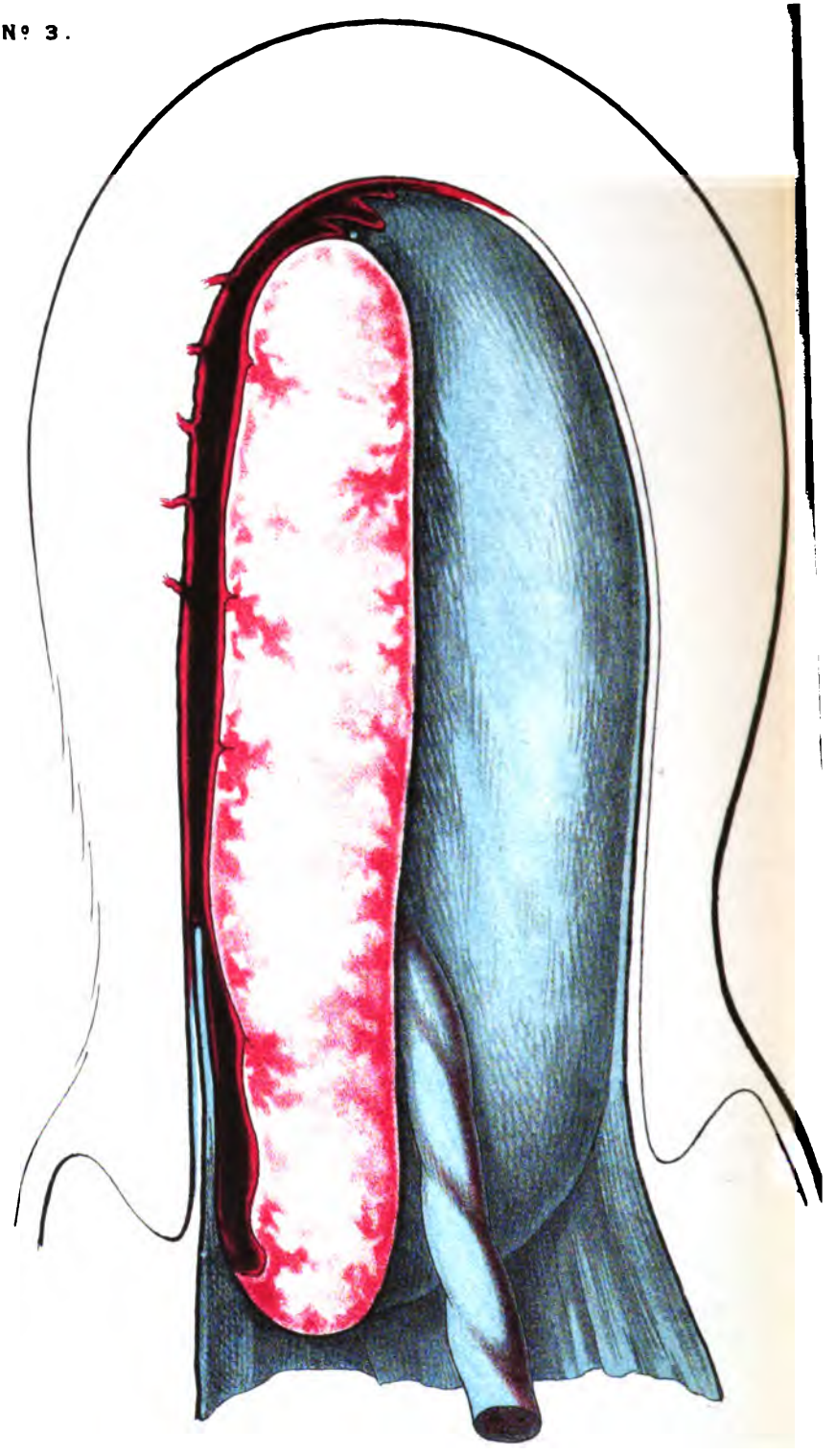
descends presenting its fœtal surface, seems to me to have arisen from observers not keeping in mind the very great frequency with which the natural mechanism of delivery of this cake is interfered with. I may say, that it is unfortunately the rule to interfere with the part of the natural mechanism of delivery. Such interference, generally carried out as it is by pulling the cord, produces an unnatural mechanism—inversion of the placenta, as Ramsbotham calls it; and this unnatural mechanism, this inversion of the placenta, comes to be described as the natural conduct of the delivery. The placenta, says Rigby,* "descends into the vagina inverted, *i.e.*, with its fœtal or amniotic surface turned outwards. Whether or not," he adds, "this is produced by pulling on the cord, is perhaps a question."

To find out the natural mechanism of the expulsion of this cake, it is only necessary to watch the process as nature conducts it; that is, in cases in which the practitioner does not try to modify it in any way. This any one can easily do, by wounding or otherwise marking the part presenting at the mouth of the womb, and then after its birth examining the placenta to find where the wound is; or the observer may pinch the part first presenting at the vaginal orifice, and retain hold of it till the whole is born, and then find what is the part so pinched.

In this way it is easily discovered that the part of the placenta presenting at the os uteri, and subsequently at the os vaginæ, is not the fœtal or amniotic surface, but the edge of the placenta, or a point very near the edge. When it is not exactly the edge, the placenta is not inverted or folded upon itself, there is only a little of the lower marginal part of the cake transversely folded up, as I have depicted in the third plate; it is still really the edge that presents, only thickened a little by being folded on itself; and I think this folding occurs chiefly in placentæ which are thin at the part folded. This folding is manifestly caused by the pulling up of the edge by the

* System of Midwifery, p. 103.

Nº 3.



SEPARATION & PARTIAL EXPULSION OF PLACENTA.

Matthews Duncan.

still adhering membranes; the resistance of the force required for their separation being greater than the rigidity of the marginal part of the placenta so folded.

My own numerous observations satisfy me that the inversion of the placenta, or its folding upon itself transversely to the passage, or the presentation of its fœtal surface, as authors describe, and as Schultze and others depict, is a very rare occurrence,—so rare as to debar describers from calling it *a* natural, and still more from calling it *the* natural mechanism. The placenta is folded upon itself during the process, as I have attempted to depict in my drawing; but the folds are according to the length of the passage, not transverse to it, as inversion or presentation of the fœtal surface imply. (*See* Plate 3.)

These remarks are remarkably corroborated by the memoir of Dr Lemser on the Physiological Separation of the Placenta. This gentleman's observations appear to me to be inconsistent with the views of Baudelocque and Schultze as to the separation of the placenta, and they are, undoubtedly, the best observations on the point which we possess. But it is more interesting, with a view to the present discussion, to remark that Lemser always describes the border or edge of the placenta appearing in the os uteri as the ordinary result of separation and propulsion of the cake.

The advantages of the natural mechanism, as I have described it, are obvious. It is true that, after the passage of the bulky child, there is no such necessity for a mechanism of the delivery of the comparatively small placenta as there is for the passage of the child's head. There is ample room and verge enough for the placenta passing in any way. But the natural mechanism claims respect as the *natural* mechanism, and, moreover, it presents obvious advantages over any other mechanism. Just as the child's head passes through the pelvis so as to dilate the passages as little as may be, or in the manner demanding least expenditure of force, so also does the placenta. It comes edgeways. If it came inverted, or

transversely doubled up, or folded into a cup shape, we should have a body passing that required at least twice as much space as is required if it passes edgeways, and only longitudinally folded. But this is not the only advantage of the natural mechanism.

If the placenta is expelled as Baudelocque describes, and as Schultze depicts, then a loss deserving the name of a hæmorrhage is almost as necessary as it is certainly a generally described accompaniment of the process of the expulsion of the placenta. For the placenta has a certain amount of rigidity, and its folding on itself and the forcing of it into a cup-like shape cannot be effected without a hallow space being offered for the reception of blood, or indeed without a certain force being exerted to produce the folding and a vacuum, which force will also tend to draw blood into the said hollow from the open uterine sinuses which were in apposition to the part folded. Baudelocque's descriptions and Schultze's drawings, while they do not give what is natural, yet do indicate a mechanism of which they justly make considerable hæmorrhage a necessary or nearly necessary part. It would be easy to show great, if not insuperable, difficulties in the way of accepting Baudelocque's description of this mechanism. It is enough for me to assert that it does not exist as a fact, save as a rare exception to the ordinary process. I say no more, because I have no intention of entering here on the subject of hæmorrhage during the detachment and expulsion of the placenta.

According to Baudelocque and Schultze, the folding is always on the uterine surface. But this is far from being the case. According to Lemser,* it is more frequent than folding upon the fœtal surface. In my drawing, I have represented the placenta, with a view to pictorial facilities, as folded upon its fœtal surface.

If the placenta comes edgeways, its uterine surface glides along the surface of the uterus; its foldings, parallel to the length of the maternal passages, are well squeezed

* Die physiologishche Lösung des Mutterkuchens. Giessen, 1865. S. 17.

together, and little space is offered for the reception of blood flowing from uterine sinuses. The uterine wall keeps close to the folded placenta. The uterus contracts, forces the placenta downwards, and at last its body is nearly globular and empty. There is no hæmorrhage worthy of the name. Hæmorrhage, when it does occur, is not demonstrated to take place according to the description of Baudelocque or the plate of Schultze; and I believe these gentlemen do not give the correct account of it. Authors too frequently, I may say almost invariably, describe too great an amount of hæmorrhage as part of this natural process. I admit that a frequency of some hæmorrhage is a strong argument in favour of this proceeding. But I believe that interference, which, though common, is frequently injudicious, is occasionally the cause of this hæmorrhage, which is, therefore, in such circumstances, unjustly laid to the account of the natural mechanism. It is far from uncommon to observe labours in which there is no hæmorrhage, in which not an ounce of blood is lost during delivery, there being only enough to smear the uterine surface of the placenta with a very thin layer. This absence of hæmorrhage I regard as the natural state, and in this I suppose all obstetricians will join me, at least if I introduce the element of desirableness as an indication of naturalness. Such absence of hæmorrhage depends on the adoption of what I describe as the natural mechanism. The presence of hæmorrhage is a part of the erroneously described natural mechanism, and to me this presence is one proof of the erroneousness of the description.

But although the mechanism of Baudelocque and the picture of Schultze do not give the natural process, they indicate a state of matters which is frequently observed after the separation of the placenta. Schultze's second drawing is an admirable representation of what takes place frequently, perhaps generally, when any considerable force is used to deliver the placenta by traction of the cord. Then indeed, truly, the placenta is inverted, and its edge puckered up purse-like. The insertion of

the cord comes first, as is so frequently represented in woodcuts. The placenta is transversely bent on itself, and puckered up; hæmorrhage flows to fill up the partial vacuum which is thus produced. The inverted mass forms a firm plug, closely filling the vagina. Traction on this plug is exactly like traction on the piston of a pump. If hæmorrhage does not naturally take place to fill up the void which tends to be formed beyond the placenta, then it is powerfully attracted and induced by the piston-like action of the placenta pulled by the cord. The interior of the uterus, already scarified by the separation of the placenta, requires but this pulling at the cord to be effectively cupped.

From all this there follows the very valuable corollary, that in practice the third stage of labour should be left to nature, and that, when interference is required, the natural mechanism of the birth of the placenta should be as closely imitated as circumstances admit.

I shall conclude with a quotation from Cazeaux,* which shows that this practical view has not been altogether neglected:—"'When the placenta is partially engaged in the orifice (of the womb) by a portion of its periphery, this plan,' says M. Guillemot, 'ought to be somewhat modified; for in this presentation, the root of the umbilical cord, instead of corresponding to the cervix, is higher up in the uterine cavity; and hence, if the operator resorts to traction, the centre of the placenta will have a tendency to enter the orifice, and thus add its bulk to the disk already engaged there. Such a disposition sometimes constitutes an obstacle to the further delivery of this mass; but it is surmounted by making some moderate tractions, not on the cord itself, but rather on the parts previously engaged, by applying two fingers on its surfaces.' We have," adds Cazeaux, "had numerous opportunities of testing the practical utility of M. Guillemot's advice."

* Theoretical and Practical Treatise on Midwifery, Bullock's translation p. 385.

Dr Thomson said that he had noticed in a confinement case he had the other day, that the placenta came away in the manner described by Dr Duncan, and there was almost no hæmorrhage. It was quite plain, he thought, that suction was produced by pulling at the cord, and hence hæmorrhage. It is sometimes necessary, however, to use traction to get the placenta away.

Professor Simpson thought that one result of Dr Duncan's paper would be to fix the fact in the minds of practitioners, that there is always danger in pulling by the cord.

Dr Cochrane said he had never found much difficulty in removing the placenta, but was of opinion that hæmorrhage might be caused by traction on the cord.

Dr Ritchie said that hitherto he had always regarded Schultze's diagram as the correct representation of the mode of separation of the placenta.

Dr Jefferiss said, that in an experience of forty years he had always avoided traction on the cord when the placenta was still in the uterus; but when he found that it had been expelled into the vagina, he though there was no harm in pulling the cord.

Dr Bruce was of opinion that the point had not been sufficiently studied. He was in the habit of usually waiting till the placenta was quite ready to come away, and then removed it. There was almost always hæmorrhage to a certain extent, whether the afterbirth came away naturally or not. Of course, in some cases it is absolutely necessary to take it away.

Dr Sidey believed that, besides the manner of expulsion described by Dr Duncan, there was another, in which the placenta presented at its centre. He had seen a number of cases of labour where there was not as much bleeding as would colour the fingers.

Dr Gordon held that the period between the expulsion of the child and the removal of the placenta ought to be shortened as much as possible.

INCISION OF THE CERVIX UTERI, WITH DESCRIPTION OF A NEW HYSTEROTOME.

By J. G. Sinclair Coghill, M.D.

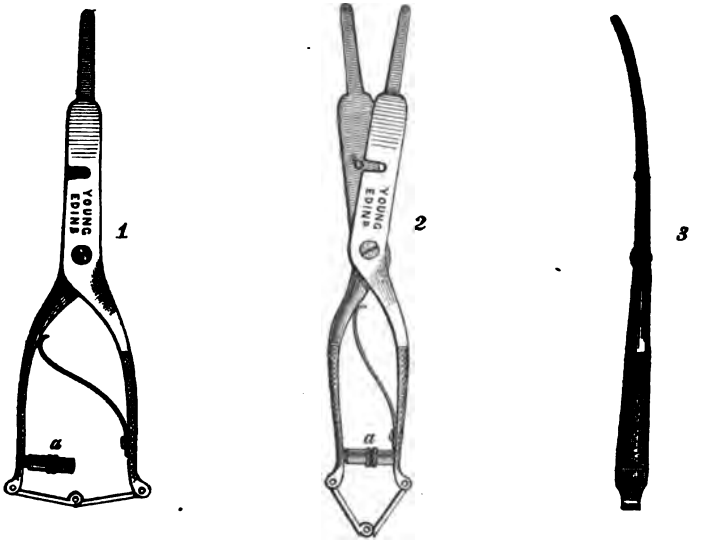
March 22, 1871.

There are few, if any, of the physical disabilities incidental to the sex, more productive of suffering, both in mind and body, than dysmenorrhœa, especially in its more persistent forms. A large, and with the advance of civilisation, apparently, an increasing proportion of women menstruate with an amount of pain and difficulty sufficient to give a decidedly morbid character to this, the characteristic function of womanhood. Monthly recurring agony, from which there is no respite, during the greater part of a lifetime is the penalty more or less of this condition. Nor are its moral contingencies less grave. Relief is sought too frequently in the narcotic influence of alcohol, opium, chloroform, and such like anodynes. On the other hand, it is unnecessary to refer to the many painful feelings and circumstances associated with childless marriage, its usual concomitant.

It is not my purpose, however, on this occasion, to enter upon the general consideration of the nature, causes, and treatment of dysmenorrhœa, but, to confine myself to such cases of painful and difficult menstruation dependent upon a strictured condition of the canal of the cervix uteri, capable of treatment by incision, or rather bilateral division. By many authorities this operation has come to be regarded with disfavour, mainly, if not entirely, on account of the risks attending its performance. I have had the opportunity of observing a large number of cases of strictured cervix operated on by the late Sir James Y. Simpson, with his well-known hysterotome, I have also frequently had recourse to this operation, with the same instrument, in my own practice, during the last fourteen years, and I have completely satisfied myself that the bad

consequences, urged so strongly as a plea for its abandonment, have resulted almost entirely from the imperfect character of the instrument employed in dividing the uterine tissues. Hæmorrhagè, often to a rapidly fatal extent, both at the moment of incision and secondary, and subsequent inflammation of the uterine and adjacent tissues involved, have almost uniformly resulted, I am convinced, from the employment of an instrument, the extent and direction of the cutting range of which it is impossible, even in the most practised and skilful hands, either to guide or control.

In the hysterotome which I have the honour to shew you (woodcuts Nos. 1, 2, 3, annexed),* I believe I have



succeeded in producing an instrument possessing all the merits claimed for its predecessors, some of which I produce for the purpose of comparison, and at the same time having a simplicity and certainty of action, which not only renders it more efficient as a mere cutting power, but, at the same time, enables the operator to have complete con-

* Made for me by Mr Young, of Edinburgh.

trol over the extent and direction of the incisions, which must necessarily vary in each case.

The instrument is on the principle of a pair of scissors, with reversed action, *i.e.*, cutting outwards by compressing the handles. In figure 1, the instrument is represented as ready for introduction. The blades overlap each other, so that the cutting edge of the one is within and protected by the rounded back of the other, while the points being blunt, the blades in this relation form a probe and director for themselves. The jointed cross-bar at the extremity of the handles, controls the direction of the blades when separated by the approximation of the handles, and which again are steadied by the spring passing between them serving as a counterpoise; at *a* is a nut and screw by which the extent to which the blades can be separated is regulated. In figure 2, the instrument is represented after introduction, when the blades are separated to an extent regulated by the screw *a* immediately previous to being withdrawn. The cutting edges are now exposed, and by grasping the handles firmly and withdrawing the instrument steadily, the suddenly diminished resistance proclaims the complete section of the uterine tissues, the compression of the handles being then relaxed, the blades return at once to their mutual shelter by the action of the spring between the handles, and the instrument can be safely removed from the vagina, the cutting edges being thoroughly protected. In figure 3, the instrument is represented in profile. It will be observed to have the general curve of the ordinary uterine sound. This both facilitates its introduction and adapts it to the inclination of the axis of the uterus within the pelvis.

If this instrument is compared with Sir James Y. Simpson's hysterotome, its advantages, both as regards facility in use and certainty and efficiency of action, will be at once apparent. The latter consists of a single straight lancet caché, the first incision with which may be made with tolerable ease. To complete the operation, however, the instrument has to be again introduced with the edge

of the blade turned to the opposite side; but the first incision has of course destroyed the fulcrum or support in the cervical wall, necessary for the complete and exact performance of the second incision, which is accordingly almost invariably irregular in extent and direction.* This is the source of frequent trouble, both at the time of operation and subsequently, and the same objection applies to all single-bladed instruments. I have used myself only one form of hysterotome, of two blades, but on the lancet caché principle—but the thinness of the blades rendered necessary by the metal director, within which they were concealed, permitted them to bend so much as to make the incisions uncertain and irregular; while the complicated mechanical arrangements, by which the movements of the blades were controlled, rendered the instrument difficult to manage, difficult to clean, and extremely liable to get out of order. During the past year I have used my hysterotome in six carefully selected cases, with the most perfect ease and success,—indeed, its simplicity of action is such as to enable the most unpractised hand to use it with complete safety and confidence.

There are several points in connection with the management of the operation which I think of great importance. The time selected for its performance should correspond to the sixth or seventh day after menstruation has ceased.

* If the incisions do not correspond to the bilateral plane of the pelvis, the peritoneum is apt to be divided, and the inflammatory action lighted up, ends frequently either in fatal peritonitis, or pelvic cellulitis and abscess. Again, if the incisions are made too deep and involve too great an extent of the uterine wall, violent hæmorrhage comes on—most difficult to arrest, and which, I have more than once seen, terminate fatally.

With my instrument the incisions must necessarily be opposite each other, while the relation of the handles externally, will determine their being perfectly bilateral as regards the plane of the pelvis. The incisions being made from within outwards, the necessary resistance is made by the cervical wall for both at the same moment, and ceases at the same time by its equal division. Again, the depth and length of the incisions can be exactly fixed by the regulating screw, which arranges to what extent the handles can be closed, and a corresponding separation of the blades effected.

This gives the benefit of the depletion, and avoids the possible embarrassment of the last period, and what is still more important, allows sufficient time for the divided surfaces to heal before the onset of the next. The length, shape, and density of the cervix uteri should be carefully noted, together with its angle of inclination, and that of the uterus in the pelvis, in order that the corresponding adaptation of the instrument for the necessary incisions may be effected. The position of the stricture, which may involve the whole or only one point in the cervical canal, is not so important, as the instrument should always be introduced so far within the canal as to accomplish the incision of the cervical wall bounding the os internum, the most common site of stricture. The incision commencing immediately above that point, should be widened gradually as the os externum is reached so as to terminate at that point by complete bi-section of the cervical wall. In most cases it will be necessary previously to dilate the canal with a small tangle tent, and I am in the habit of using them hollow, slightly curved, and three inches long. In this form they are most convenient, as they pass into the canal readily, and require no forceps or holder for their introduction, while their withdrawal is as easily effected.

The after-treatment is most important, for if the parts are allowed to remain undisturbed, cicatrization with increased contraction rapidly follows. I am in the habit of passing a small bougie or suppository of perchloride of iron within the incised canal, and introducing a large pessary of the same nature, which I bruise against and allow to remain between the divided lips of the cervix uteri. It is necessary to pass up the finger daily to separate the divided surfaces and prevent their adhesion. The indications for general treatment are most simple—rest, avoidance of exertion and excitement, and a light cool diet.

I should state in this connection that I consider this operation only justifiable in cases of strictured cervix,

where the various means of dilation have been found unsuccessful, which is generally the case in the long nipple-shaped cervix, where there is apparently hypertrophy of the elastic tissue, which presents permanent results from the dilating process. I would also direct attention to the fact that many cases of the membraneous dysmenorrhœa, of chronic inflammation of the uterus, and the well-known condition of irritable uterus, are frequently met with, and found to be stricture of the cervix, which, obstructing the menstrual flow, leads to congestion and its sequelæ, to uterine hyperæsthesia, and other allied conditions, and which the operation of hysterotomy will be found to cure.

Dr Bell was of opinion that the instrument shown would make a very short cut into the internal os. And if a tangle tent dilated the os, why should there be any cutting at all? He believed that constriction took place even after the operation. The secret of this operation consisted in its depleting the neck of the womb, and thus relieving inflammation. The most serious objection to the operation was the tendency to inflammatory action afterwards.

A SKETCH OF THE MECHANISM OF THE DELIVERY OF THE CHILD, COMPILED FROM NOTES OF DR MATTHEWS DUNCAN'S LECTURES.

By JAMES RITCHIE, M.D.

(*Communicated by Dr Duncan.*)

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IN the following paper, reference will be made only to the commonest position of the child in labour, the left occipito-anterior. As the mechanism of delivery is first exhibited in somewhat difficult first labours, and is most advantageously studied in such a connexion, the following sketch will refer to such a labour completed by the natural powers.

It is necessary in the study of this mechanism to notice,

1*st.* The propelling or driving power.

2*d.* The body to be moved.

3*d.* The passage through which it has to be driven.

An exhaustive account of each of these is not required, but only so much as has a direct bearing on the subject on hand.

1*st. The propelling power.*

The expulsive force consists of, (*a.*) Chiefly the contractions of the muscular fibres of the uterus, which contractions are called "pains;" aided in the stage of fœtal progress by (*b.*) The assistant expulsive powers, viz., the contractions of the muscular walls and roof of the abdomen, which contractions are called "bearing-down."

(*a.*) The true pains—as distinguished from spurious and non-effective pains, with which we have nothing to do—vary in power from what is just sufficient to move the child up to 80, or perhaps even 100 lbs. In such a labour as we are considering, the power probably varies from 20 to 40 lbs. The characters of these pains are, that they come gradually into action and attain a climax; that they then gradually subside; and that each pain is followed by an intermission. As labour advances, the pains increase in length, strength, and frequency; the interval diminishes in length, but it is better marked by freedom from contraction. The duration of a pain varies in length from less than half a minute at the commencement of labour, to several times that length at the time of delivery. The increase in strength is due to the greater thickness of the walls as the uterus diminishes in bulk, to the then smaller curve of its walls, and to the co-operation of the voluntary muscles. The hard irregular surface of the fœtus, after the discharge of the waters, being more irritating than the distended smooth membranes are before their rupture, may tend to increase the strength and frequency of the pains. When the child makes a considerable advance—or, to describe more correctly, when the child has been

expelled a considerable way by one pain—there is a lull in the pains, for it takes some time for the uterus to contract permanently, and accommodate itself to its diminished contents, so as again to act with energy on the fœtus.

(*b.*) The bearing-down efforts are brought into play during the uterine contractions voluntarily, or by reflex action after the head has reached the walls of the vagina. There is no means of separately estimating their exact value. They merely aid the uterus by mediately causing a pressure on its walls, which is equal on all the unattached abdominal surface of the organ.

The value of the intermittent nature of the pains is great, both to mother and child, by giving to both a rest at intervals, by preserving the textures from that destruction which would be the result of constant pressure, and by permitting the placental respiration of the child to be perfectly resumed in the intervals.

2d. The body to be moved, viz., the child.

The head is the largest part. Wherever it passes, the rest of the body will pass. It is nearly oval in every section. The greatest diameter involved in vertex cases is the occipito-frontal, which measures 4½ inches. This diameter is supposed to be normally at right angles to the vertical median line of the child's body, but when the head is flexed the diameter, which is then at right angles to the vertical in the occipito-bregmatic, 3½ inches. The greatest transverse diameter is the biparietal, which measures about 3½ inches, but the transverse diameter a little in front of this, the bitemporal, is shorter, and measures about 3 inches.

The downward power exerted by the uterus on the head is, for the most part, communicated by way of the bony vertebral column; and as the articulation of the vertebral column is in most cases nearer (in some rare cases it is not so) to the occipital than to the frontal extremity of the occipito-frontal diameter, making the ante-

rior lever arm longer than the posterior, resistance is therefore more favourably applied at the sinciput than at the occiput.

The bones of the fœtal head are not rigid, like those of the adult, but may be greatly bent by pressure. At the sagittal suture the bones are partly separated by membrane, chiefly at the anterior fontanelle; and where they are in contact they are not inseparably united by dovetailed margins; consequently pressure on the sides of the head causes equitation of the bones and a reduction in the transverse diameters of the head.

The fœtal head admits of more free movement on the trunk than is possible in the adult, and may be rotated at least to the extent of a full quarter of a circle to either side without injury.

3*d. The Passage.*

The walls of the passage are not of the same nature throughout. They are at first bony, then composed of bone and ligament, and lastly, are composed, almost entirely composed, of soft textures. The passage, down to the middle of the third sacral vertebra, is straight; its axis corresponds with that of the uterus, viz., a line from the navel to near the tip of the sacrum. Below the third sacral vertebra it is a curved passage, with the curvature in the antero-posterior median plane. The direction of the axis therefore varies from point to point, and describes an arc of a circle (circle of Carus) of which the centre is behind the pubic joint; when the passage is distended the axis emerges from the woman's body in a direction nearly directly forward, supposing the woman to be in the standing posture.

The brim is formed entirely of bones, united at three points by synchondroses. Its greatest diameter, the bisiliac, measures about 5 inches; the smallest, the conjugate, about 4 inches; the oblique is intermediate, about 4½ inches.

The outlet of the ligamentous pelvis is partly bony,

partly ligamentous; a joint, the sacro-coccygeal, also enters into its composition; its measurements contrast with those of the inlet; the antero-posterior with the coccyx extended being the longest, about 5 inches; the transverse, the shortest, 4 inches; and the oblique intermediate.

When these parts are clothed with the soft textures, the above measurements are reduced only to a small extent. Of this portion of the passage the lateral walls converge from above downwards, the anterior and posterior diverge. The direction of the axis of the outlet is in a line passing from the promontory to the middle of the perinæum; hence anything fully occupying the outlet of the ligamentous pelvis, instead of tending to come out through the vulva, strikes upon the perinæal part of the floor of the pelvis.

The outlet from the woman's body, unlike the inlet and outlet of the pelvis, is not naturally a large opening, but requires to be greatly distended before the child can pass through it. In the undistended state, the perinæum, which forms the posterior wall of the passage (*i.e.*, of this part of it), measures only from 1 to 1½ inches in length; but when distended so as to allow of the passage of the fœtal head, it measures about 4 or 5 inches, or nearly half of the whole posterior wall of the genital canal. The outlet from the woman's body may assume any form, inferiorly or posteriorly, being composed entirely of soft textures, but above it is limited by the pubic arch anteriorly.

From the preceding statement of facts it is evident, that in size the fœtal head bears a very close relation to the dimensions of the inlet and outlet of the pelvis as clothed with soft parts, that in order that it may pass through these it must travel in a definite manner, and that before it can be born into the world it must enormously distend a part of the passage.

Changes before Labour which greatly influence the Mechanism of Delivery.

During the last days of pregnancy, the whole of the pelvic structures become infiltrated with a peculiar juice, rendering them soft, expansible, and easily moulded by pressure exerted upon them; the intrinsic joints of the pelvis are relaxed, permitting of a sliding motion at the sacro-iliac synchondroses, and at the symphysis pubis; the vulva, vagina, and cervix uteri become relaxed; and when labour is at hand there flows from the vagina and cervix uteri a copious glairy discharge. The softening of the cervix, which proceeded from below upwards, was completed so long since as about the middle of pregnancy; but there is a further dilatation of the cervix from above downwards, which takes place during the last few days, or hours only, of pregnancy.

Changes during Labour.

At the beginning of labour, the exact time of which is unknown, the uterus is situated in the cavity of the abdomen, generally inclined towards the right side; its fundus is above the navel, its lower end bulges more or less into the cavity of the pelvis, resting on the brim. Its axis may not correspond exactly with the axis of the brim, but during a pain the uterus is erected, so that its axis then more nearly coincides with that of the brim of the pelvis. We may therefore, for practical purposes, consider that the force is applied to the contents of the uterus in a line corresponding with the axis of the brim. This is not strictly true, for the lateral obliquity of the uterus may be so great that it is not obliterated by the erection of the organ. In such a case there will be formed a second curve in the genital passage, a curve in the transverse mesial plane. If the lateral obliquity be to the right, the concavity will look towards the right; if the lateral obliquity be to the left, the concavity of the curve will look to the left.

In the position we are considering—viz., the first

position or left occipito-anterior—the fœtus is lying in the cavity of the body of the uterus, with the occipito-frontal diameter of the head related obliquely (obliquity of Solayres) to the conjugate and transverse diameters of the pelvis, but corresponding more nearly to its right oblique diameter, the occiput being to the left and anterior. The presenting point—*i.e.*, the point of the vertex through which the axis of the genital canal passes —is in the sagittal suture.

The driving power, as already stated, is at this stage the uterine contractions only, exerted upon the mature ovum in the direction of the axis of the brim of the pelvis. The obstacle to be overcome is the partially dilated cervix uteri. The force is exerted upon the cervix through the medium of the bag of waters, which renders the pressure equal at every part. The dilatation takes place easily in consequence of the relaxation of the muscular fibres. The beginning occupies the longest time. When the os has attained the size of a florin, the dilatation proceeds much more rapidly. As the os increases in size, a portion of the bag of membrane is protruded, and whereas at the beginning of labour they were felt to be in close relation to the fœtal head, they are now pushed downwards and separated from it—a change which has, perhaps, given rise to the notion of repulsion of the child at the beginning of a pain. The protruded portion of the bag of membranes becomes quite tense during each pain. As the os dilates, a larger portion of them is protruded, and (unless the quantity of liquor amnii be very great) the head is pressed against the dilating cervix, and through the medium of the wall of the uterus upon the brim of the pelvis, unless the brim of the pelvis be large in comparison with the head. The bag of membranes generally bursts when the cervix is nearly dilated, but the time depends very much upon the strength of the pains compared with that of the membranes, the relative size of the passages and head, upon the character of the pains, and upon the condition of the os.

As we have already seen, the occipito-frontal diameter of the head may be considered to be a lever on which the uterus exerts a pressure—not at its middle point, but nearer to the occiput than the forehead, and the resistances offered by the soft parts to the occiput and sinciput at the extremities of the lever being nearly equal, consequently the occiput tends to descend in advance of the forehead, and there is frequently produced what is called the Rœderer obliquity—viz., flexion of the fœtal head upon the trunk. After the head occupies the pelvis, the presenting point leaves the sagittal suture for the right parietal bone, and the presentation is no longer direct but oblique; and it is the occipito-bregmatic diameter of the head which is at right angles to the axis of the passage. As that diameter is shorter than the occipito-frontal, the head passes through a smaller opening than it would do if the latter diameter continued to confront the difficulty.

The head is in general forced prematurely through the os—*i.e.*, before it is thoroughly dilated, causing a tear in the cervix, most commonly at the left side, probably because the occiput is first forced through, and in this position—the most common one—occupies that side; probably, also, because of the right lateral obliquity of the uterus.

If the brim of the pelvis be large compared with the fœtal head, the head may have had only the difficulty of the dilatation of the os uteri to contend with, and may have entered a considerable way on the second stage before completing the first, but this is by no means always the case. The occipito-frontal diameter occupies a right oblique diameter of the pelvis. The conjugate diameter of the pelvis, which is that at which the difficulty almost invariably occurs, is occupied not by the bitemporal, but by a diameter which passes with a slight obliquity from the hollow in front of the left parietal bone, to a little in front of the right tuber parietale. If the pains be strong, the head may be forced through when the brim is so

much contracted that the promontory makes a groove on the left temporal and parietal bones; equitation of the bones may also take place, but it is not here that we have the most common cause of equitation, as shall afterwards be explained.

When the head has passed both the os uteri and the brim of the pelvis, it may pass at once down to the perinæum, the walls of the vagina being, to a certain extent, dilated and easily dilatable, or it may advance slowly during each pain.

The retrocession which occurs when the head is far advanced is caused by the retentive power of the abdomen, for the muscles of the abdominal walls and the diaphragm are now aiding the uterus by bearing down during each pain; in the intermissions they cease to act, and, in returning to their state of rest, suck back the uterus and fœtus a short way. The motion of recession is most marked when the head has reached the perinæum, and has somewhat of the same value as the intermissions of the pains, by withdrawing the head from active compression by that portion of the soft textures which has been most recently and only partially distended.

The great obstacle to the further passage of the head is the undistended perinæum, rendering the moulding of a passage necessary.

It may be well here to describe a tumour formed on the child's head, which, although it does not materially affect the birth of the child, is nevertheless a result of the mechanism. As long as the ovum is entire, it can be shown that the child is, during a pain, subjected to a pressure which is equal at every point of its structure, analagous to the atmospheric pressure on every living being; but as soon as the bag of membranes has been ruptured, and the waters have been discharged, that part of the body which corresponds to the os uteri, or to the lumen of the vagina, is subjected to the atmospheric pressure only; but all the rest of the body is acted upon by the atmospheric pressure, plus the force of the labour

pains. There is thus produced a condition of things similar to that seen in cupping, but brought about by the converse process—*i.e.*, instead of the pressure over the part affected being reduced below that of the atmosphere, the pressure on all the rest of the body is increased beyond the atmospheric pressure by that of the labour pains. The result is analagous to that of cupping—viz., the forcing to the point of least pressure of a quantity of fluid—in this case bloody serum—causing a tumour, to which has been given the name of caput succedaneum; but it may be formed on any part of the body. In the first position and vertex presentation, the caput succedaneum is always on the head. If the waters are discharged early it will be formed on the sagittal suture, extending a little further over the right parietal bone than over the left, by reason of the oblique insertion of the cervix into the vagina. Such a caput succedaneum is called of the first stage, but is seldom formed; and, even if formed, is in the majority of cases nearly obliterated by that of the second stage. The caput succedaneum most common is that of the second stage; it is formed on that part of the head which corresponds to the lumen of the vagina while the perinæum is being distended—viz., the upper and posterior part of the right parietal bone, or near this.

Before proceeding to consider the caput succedaneum, we left the head distending the perinæum. This takes place by expansion of its component structures, and is a work mainly of forcing. The expansion is greatest where the force is greatest and the tissues are weakest—viz., in the median line. The driving power acts as if with the intention of forcing the head through the perinæum, which it sometimes succeeds in doing, notwithstanding the support rendered by the obstetrician. Down to the transverse sectional plane passing through the middle of the third sacral vertebra, the presenting point is in the sagittal suture, or nearly so, because the antero-posterior mesial plane (with reference to the child only) of the child's head—in which plane the head is flexed—corre-

sponds with the axis of the brim; but from the above point onwards the presenting point is gradually shifted in an oblique direction over the upper and posterior part of the right parietal bone; that is to say—resolving the oblique line into its component transverse and antero-posterior displacements—the right or anterior half of the head is now in advance of the left, causing the transverse shifting of the presenting point, and the occiput has advanced by rotation towards the symphysis, the face passing backwards towards the hollow of the sacrum, causing the shifting of the presenting point posteriorly towards the occiput. The mechanism producing these changes respectively is as follows:—

(*a.*) That which causes the right half of the head to be in advance of the left.

Because, though the direction of the force continues the same, the axis of the passage no longer corresponds with that direction, and the driving power sends forward the head parallel, or nearly so, to its own direction, the head descending to the resisting coccyx and perinæum; consequently the circle of Carus passes not through the sagitta—which can be felt above the coccyx when the head has been long in performing its rotation—but through the right parietal bone.

(*b.*) That producing the rotation of the head on its vertical axis.

As was already pointed out, the lateral walls of the pelvis converge towards the outlet, making the transverse diameter the shortest; on the other hand, the anterior and posterior diverge, making the antero-posterior diameter of the outlet the longest. It has also been noticed that, on account of the shortness of the posterior part of the cranial lever, the posterior part of the vertex leads; further, the anterior part of the passage is at this stage most developed; consequently, as the head descends in the pelvis, the vertex passes forwards to occupy the part at which the resistance is least guided by the convergent lateral walls, and the head is caused to rotate in the pelvis,

so that its long diameter may occupy the long diameter of the outlet. The rotation, in the first position, is less than a quarter of a turn; and the occipito-frontal diameter of the head generally does not pass so far as to correspond with the antero-posterior diameter of the outlet.

The next motion of the head is a decrease of flexion, produced by the necessity of advancing along the curvature of the genital canal, in the antero-posterior vertical plane. The resistance to the passage of the head is almost entirely in the posterior wall of the canal. The head being more forcibly pressed against the concavity, there is of necessity a greater amount of friction at that part. The friction is of comparatively little moment, however, for the parts are well lubricated; but what is of more importance, the development of the passage is greatest at the posterior part of the canal, and the development is the great cause of resistance. Although the line of direction of the force of the uterine contractions remains the same, and corresponds with the axis of the brim, the point of the head to which that force is directed is no longer in the transverse mesial plane of the brim—the plane occupied by the line of the direction of the uterine force—but is by the advance of the head moved gradually anterior to that plane, consequently the line of the force, as applied to the head, emerges from the perinæum no longer at the coccyx, but anterior to it.

Through the mechanism described in the preceding paragraphs, there is produced, in the fœtal head, a shear, which has been considered as made up of two parts—a vertical shear, and a lateral shear.

The vertical shear, as seen after the birth of a child, consists of the flattening and displacement downwards of the left parietal bone, and the bulging and displacement upwards of the right parietal bone, which also often overlaps its fellow. The lateral shear consists of the pushing forwards of the left side of the head, and pushing backwards of the right side; the terms upwards, forwards, etc., being used *quoad* the child and not the mother.

Both of these shears have the same cause, and the

predominance of the one or the other depends upon the time at which the rotation of the head takes place. As the head descends in the pelvic canal, the resistance in the anterior wall, as already shown, diminishes; and passing from the anterior wall backwards, it gradually increases; the maximum of resistance being in the posterior wall, and in the lower part of that wall. The driving power forcing onwards the head, that part of it which is opposed by the resistance of the posterior wall is flattened, and at the same time pulled back as it were; while the opposite side of the head, the right side, not having the same amount of resistance to contend with, bulges out, and is pushed forwards over the posterior or left half, so causing a vertical shear, and often also equitation of the bones.

Then while the rotation is taking place, and when it has been accomplished, the resistance in the posterior wall of the passage causes the holding back (*quoad* the mother) of the left and frontal region of the head, but the right and occipital region is pushed forwards (*quoad* the mother) by the driving power, and hence a lateral shear. The existence of the lateral shear was long overlooked, because it takes place in the same direction as, and is therefore masked by, the caput succedaneum.

The same forces which produce the shears cause also the overlapping of the occiput and frontal by the parietal bones, as well as the ordinary overlapping of the left parietal by the right.

The changes in the shape of the fœtal head, whether produced by compression or by serous or bloody effusions, can be safely used to confirm a diagnosis of a position of the vertex; to make a diagnosis of this point, if not already made before the birth of the child; or to correct an erroneous diagnosis made during labour.

As the perinæum becomes distended, the forehead and face, being nearer to the line of the direction of the force, as formerly described, and guided by the curve of the sacrum, move onwards over the perinæum. There is, therefore, decrease of flexion, and then extension of the head. The vertex passes forwards and distends the

vulva. This takes place by relaxation of muscular fibres, and by stretching. The occiput advances beneath the symphysis pubis, and further extension of the head takes place, consisting of a revolution round a point, which, owing to the advance of the child, is constantly shifting from the occiput down the nucha; the forehead, nose, and chin, sweeping over the perinæum, are born after the occiput in the above order. As soon as the chin has been born, the perinæum contracts considerably, rendering the motion of retrocession impossible.

There is now a rest for a short time, the uterus undergoing great reduction in size by permanent contraction. While the head was being born the shoulders were entering the brim of the pelvis, with their long diameter in the left oblique diameter of the pelvis. When the next pain comes, after the head has been born, the right shoulder advances towards the symphysis—*i.e.*, the shoulders perform a rotation in the pelvis, but in the opposite direction to that in which the head rotated. This rotation is indicated externally by the turning of the child's face towards the mother's right thigh, a motion which has erroneously been called the motion of restitution of the head. The left shoulder now occupies the hollow of the sacrum. As the shoulders advance the perinæum is again distended, the right side of the child's body is flexed, the left extended, the right shoulder becomes fixed under the symphysis, and its point is first born; but the left shoulder and arm sweep over the perinæum, and so are entirely born into the world before the right. After the lapse of a short time the breech is expelled, then the legs.

In ordinary cases, after about ten minutes, when the umbilical vessels have contracted, forcing the contained blood into the child, the cord is tied in two places, and cut between. So much for the child. But the delivery is not yet complete, and as regards the mother, almost the most important part has yet to be accomplished—viz., the expulsion of the placenta, and the closure of the utero-placental vessels.

REPORT BY THE COMMITTEE ON VACCINATION.

APRIL 12, 1871.

AFTER due consideration of the Remit made to them by the Society, the Committee are of opinion that, before taking any steps towards memorialising Government on the subject of Vaccination, with a view of obtaining amendments on the Scotch Vaccination Act, which has on the whole proved a great success, the Society should endeavour to ascertain the mind of other medical bodies and the profession generally.

They are of opinion, however, that the Scotch Act leaves too much to be done gratuitously by the medical profession; and, in fact, necessitates, in order to its successful working, a large amount of gratuitous vaccination by them, especially in large towns, and of much unremunerated labour on their part in the way of certificate writing and clerking.

FŒTUS WHICH HAD BEEN EXPELLED WITH THE MEMBRANES ENTIRE.

EXHIBITED BY ROBERT BRUCE, M.D.

APRIL 12, 1871.

DR BRUCE exhibited a fœtus which had been expelled with the membranes entire. It was about the seventh month, and had been dead for some time.

FŒTUS WHICH HAD BEEN EXPELLED WITH THE MEMBRANES ENTIRE.

EXHIBITED BY G. STEVENSON SMITH, ESQ.

APRIL 12, 1871.

MR STEVENSON SMITH exhibited a fœtus which had been expelled with the membranes entire. The fœtus was about the sixth month, and was expelled with the membranes unruptured, after one true pain. He had attended the lady in two previous confinements. In the first, the baby was born in a very weak state, but is now a strong healthy boy; and in the second, the child was expelled stillborn at the eighth month.

MEMBRANES, ETC., OF A FŒTUS.

EXHIBITED BY G. STEVENSON SMITH, ESQ.

APRIL 12, 1871.

MR STEVENSON SMITH showed the membranes, etc., of a fœtus, between the second and third month, which had given rise to much hæmorrhage, endangering the life of the mother. On examination, a clot about the size of a small marble was found, and this effusion had evidently induced the premature expulsion of the ovum.

LONG-RETAINED GUTTA PERCHA PESSARY.

EXHIBITED BY ALEXANDER KEILLER, M.D.

APRIL 12, 1871.

DR KEILLER exhibited a gutta-percha pessary, removed from a patient who had retained it in the vagina for five years. The patient had a fibroid tumour complicated with hernia. She had worn the pessary all that time without having any pain, fœtor, or discharge.

Dr Gordon remarked that pessaries of gutta-percha might be left in any length of time without giving rise to any unpleasant symptoms.

Dr Cochrane said he had met with one case in which a pessary had been retained for several years.

TWO INTERESTING CASES.

BY T. H. PATTISON, M.D.

APRIL 12, 1871.

DR PATTISON made some observations on two interesting cases which had occurred recently in his practice.

Drs Keiller and Jefferiss and *Mr Stevenson Smith* made some remarks.

A CASE OF SPONTANEOUS VERSION.

By Stanley Haynes, M.D., Helmsley.

(*Communicated by G. Stevenson Smith, Esq.*)

April 12, 1871.

At 4.30 P.M. to-day, I attended Mrs M., aged thirty-seven, and found she was in labour; that the os uteri was dilated to the extent of two inches in diameter, and its edge was thick and soft; that the membranes protruded as a fluctuating rounded tumour; that the head presented in the left occipito-anterior position, and was at the pelvic brim; that the bladder and rectum were empty, and that all the parts were in a favourable condition. The patient believed that the membranes had ruptured the previous day, but she had not had any labour pains until noon to-day; she now felt the pains in her back much more than in front. The pains recurred every three minutes, were moderately strong, and were dilating the os. The patient wishing to use a night-stool, I went into the next room, whence I was called in haste in a few minutes (at 5.15), and found her just flinging herself on to the bed, and requesting my assistance. It appeared that while on the stool she had felt a sudden and great movement within, and directly afterwards a conviction that the birth would be immediate. The breech and right foot now presented, the back of the child being towards the left foramen ovale. During the next pain, the presenting parts and the body up to the funis were expelled, and the left thigh and leg were found to be opposed to the trunk of the child. As the cord pulsated well, the next pain was waited for and quickly came, causing the birth in succession of the left foot, right arm, and left arm. Another pain completed the delivery of a moderate-sized, vigorous male child. The placenta came away twenty minutes later (at 5.40.)

The mother stated she had had seven children without

any unusual occurrence, and that all had presented by the head. During the past week she suffered much from vomiting, which was arrested by the third dose of a bismuth and hydrocyanic acid mixture.

CASE OF DEATH OF FŒTUS FROM COILING OF THE FUNIS.

By Alexander Milne, M.D.

April 12, 1871.

Mr Thomson, in the absence of Dr A. Milne, showed a drawing of a fœtus which was destroyed when near the full time, by a triple coiling of the funis. This coiling took place around the abdomen, just a little below the umbilicus, and was so extremely tight that the point of the finger could with difficulty be inserted under it. The child evidently perished from arrestment of the circulation in the cord, for it was large, and well developed, and there was no disease of the placenta. The mother said that about ten days before labour set in, the movements of the child were uncommonly vigorous, and such as to lead her to suspect that something was wrong. Probably these movements were of a convulsive character, and due to the arrestment of those blood changes continually going on between the fœtus and its mother.

Dr Pattison made a few observations on the case, and remarked on the singular appearance presented by the child, from the funis twisted round the abdomen.

Dr Frazer made some remarks on the distinction between asphyxia and apnœa, in connection with the death of the fœtus.

A PLACENTA FROM A CASE OF PLACENTA PRÆVIA.

EXHIBITED BY ALEXANDER KEILLER, M.D.

APRIL 26, 1871.

DR KEILLER exhibited a placenta from a case of Placenta prævia, in which he had delivered by turning. When called to the case the patient was much exhausted from loss of blood, and the vagina was almost filled with a clot. The placenta was not entirely over the os, and the membranes were unruptured. The afterbirth came away almost immediately after extraction of the child. This was a case where delay would have proved dangerous. He recollected at least three cases of death after version through interference being too long delayed.

CASE OF RUPTURE OF FALLOPIAN TUBE, AND DEATH FROM HÆMORRHAGE.

BY DR HEIGHAM.

(*Communicated by Dr Keiller*).

APRIL 26, 1871.

THE particulars of this case were submitted to the Society.

CASE OF EARLY PREGNANCY.

BY JAMES YOUNG, M.D.

APRIL 26, 1871.

MRS A. B. was born on the 11th of October 1855, and menstruated on the 11th of May 1869, at the age of thirteen years and seven months. Sexual intercourse took place on the 20th of November 1869, and she was married in March 1870—after the discovery of pregnancy, which must have commenced at the age of fourteen years

and one month. A large male child was born on the 24th of August 1870. The labour was protracted *fourteen hours*, and difficult, a large caput succedaneum having formed on the head. Mrs A. B. made a good recovery, but suffered severely from suppuration of one breast. She was confined of her second child in October 1871. Early menstruation is very common, but cases of such early pregnancy are very rare.

Dr Duncan said he remembered attending a young lady a long time ago in her first confinement, who was said to be under fifteen,—the father of the child being sixteen years of age.

Dr Keiller had seen a number of cases of early pregnancy, but the real age was often concealed. He remembered one patient who was confined in the Maternity at the age of fifteen. She was well formed, and had an easy labour.

Dr Cuthbert attended a case where the mother's age was between fifteen and sixteen, and she had been skipping the rope just the day before.

NOTES OF A CASE OF ANENCEPHALOUS FŒTUS.

By James Frazer, M.D.

April 26, 1871.

On the 23d February 1860, I was called to a married woman aged thirty, pregnant for the first time. I found her in the middle of the first stage of labour. I could not diagnose the presentation, being in doubt whether it was the breech or the shoulder which presented. However, as the labour advanced, the part came down more and more, and there was no doubt of its being the shoulder. I then resolved to turn the child. To aid in accomplishing this it was found necessary to put the patient very deeply under the influence of chloroform, because the uterine contractions were very strong. After the feet were brought

down, the expulsion of the child was effected in two or three minutes with very little help from me. The fœtus proved to be anencephalous, but apparently well-developed in other respects. It now forms part of the collection of Dr Matthews Duncan of this city. It was quite dead when it was born, but had evidently been alive not long before. I may mention that the woman, by her own account, had exceeded the usual period of nine months, by two or three weeks. She made a good recovery. I believe the case in itself is not very extraordinary. I mention it to the Society in the hope of bringing out information regarding the diagnosis and treatment of such cases. I should feel obliged to the chairman, Dr Keiller, and to the Members generally, for their opinion respecting the propriety of turning in the case just mentioned. I should like to know whether they believe that nature would have effected the expulsion of the fœtus shoulder downwards, and if she could, whether the result was likely to have been as favourable to the mother as that from the practice which I adopted.

Dr Duncan said he recollected the case, but he had seen nothing of the labour. He did not wonder at any one being puzzled with such a case, but he was of opinion that an anencephalous child might be born in any way. If the shoulder came down and there was not sufficient room to let the fœtus pass in that way, there could be no harm in turning.

Dr Keiller remarked that the diagnosis of such cases was often difficult. They were not at all uncommon. He had about a dozen specimens in his museum. In one case which occurred in the Maternity, the child lived for some time.

Dr Bruce thought the diagnosis of such cases was always a little difficult, especially till the presenting part came well down.

Dr Ritchie believed that in anencephalous cases the child was sometimes retained longer in *utero* than usual, and when the vertex presented, the second stage of labour was apt to be protracted from delayed expulsion of the shoulders.

Professor Simpson said that in some such cases the fœtus

was not at all well developed, especially when a cleft in the spine existed. The diagnosis was usually rendered easier by combined external and internal examination.

Dr Young said he had related the particulars of a case of this kind to the Society some years ago. The mother had previously given birth to two children, and since then she had had two stillborn children.

CASE OF FIBROID POLYPUS OF THE UTERUS, WITH PREPARATION.

By David Thomas, M.D., Ystalyfera, Swansea.

(Communicated by Professor Simpson.)

May 10, 1871.

Maria E., æt. 40 years, tall, and of light complexion; married eleven years; has had four children; two died young, but the two living ones are healthy, the younger now six years of age.

There is nothing striking in her antecedent history. Her father died about six years since from heart disease; mother is still alive, and has always enjoyed good health, with the exception of being occasionally troubled with rheumatism. Has four sisters and three brothers, all healthy.

24th April 1869.—I was asked to see her in consequence of an alarming attack of flooding, which had come on suddenly whilst washing the floor. She stated that she lost some blood about a fortnight before, and even on four previous occasions, but never considered it bad enough to seek advice. She has never had a miscarriage, and has always menstruated regularly up to about three months back, when the irregularity as to time commenced, and the flow to increase in quantity. On my first visit, I made no vaginal examination, simply ordering complete rest, gallic acid with brandy, according to circumstances, etc.

The following day (25th), she was much better, having rallied considerably.

26*th*.—Not so well, with a recurrence of the bleeding. On my proposing an examination, she told me that about three months back, she perceived a "hard lump in the bottom of her belly," and, on placing my hand on her abdomen, I could feel distinctly a hard smooth resisting body, emerging considerably above the pubes, with another a little nearer the umbilicus, and rather to the left side, but of a much smaller size, and evidently more superficial than the lower one. This tumour grows evidently from the thickness of the uterine wall, projecting upwards in the left hypogastric region.

In making a vaginal examination, I felt an oval body protruding through the os. It was hard, smooth, and attached pretty high up to the posterior part of the cervix uteri. By manipulating with one hand in the vagina and the other on the abdomen, one could form a fair idea of its oval size. The patient lost this time a great quantity of blood, and I was afraid she would not rally; however, she improved gradually, and by the 28th reaction had fairly set in.

Owing to its large size and its extensive attachments, I told the friends that I was of opinion that any interference with the view of its removal would be too hazardous in her present prostrated state. She continued to improve, and after about six weeks' attendance I quite lost sight of her, until 15th December 1870 (nearly twenty-one months since the date of my first attendance, and about two years since she had first noticed any enlargement), when I was again requested to see her. She was now in great pain, and, on examination, I found the tumour filling up the vagina, with its apex all but projecting beyond the vulvar aperture; and owing to the pressure signs which rendered defecation difficult and micturition impossible, I had no other alternative but to recommend a speedy removal.

Mr John R. James, surgeon, administered chloroform, and, without much difficulty, I managed to get the chain of the écraseur round the pedicle, which was very tense, and about the size of an ordinary female's wrist.

After tightening the chain to an extent short of cutting into the texture of the pedicle, we found it failed to make any further impression upon it. We could by main force work the handle up and down, but the spring would not catch—something very much after the fashion of a screw that has been over-tightened, so that the thread gives.

The écraseur, after a fruitless trial of nearly an hour had to be abandoned, and next I tried to tear it through by means of a strong whip-cord, which, like the écraseur, only proved useless. Lastly, I tied a piece of strong cord as tightly as I could and as high round the pedicle as I could reach, and below this managed with some little difficulty to cut through it with a small scalpel, and without any loss of blood. The ligature came away on the third day in some Condy's fluid, with which I cleansed the parts for four days after the operation.

The tumour, shortly after removal, weighed all but two pounds, and resembled very much in size, shape, and appearance a bullock's heart. The woman made a very rapid recovery, and in a fortnight was about the house attending to her usual duties.

Two months after the operation, I again examined the patient, and especially the uterus, but could detect nothing abnormal, beyond a small parietal tumour, situated in the fundus uteri, and rather to the left side.

Dr Simpson showed the tumour, which on section was seen to be a beautiful specimen of uterine myoma or fibroid tumour of the uterus that had originally been intramural, and, under the muscular contractions of the walls, had been partially expelled. The mucous membrane was thick and vascular, and contrasted with the paler glistening fibroid structure of the interior of the mass.

Dr Keiller remarked that the cut surface of the polypus resembled mahogany. He was inclined to think that the author was a little too timid as to cutting, after compression had been made by the écraseur. He had frequently in his own practice snipped across the pedicle with scissors or a bistoury, after it had been well crushed, and almost no hæmorrhage followed. He had exhibited to the Society

some years ago a large polypus which he had removed, about the size of the fist, and in that case he cut through the pedicle after compression with the écraseur. He had in his museum another preparation showing a polypus just coming through the cervix. An attempt had been made to remove it, but without success.

Dr Gordon remarked that in such cases the blood usually came from the uterus, and not from the tumour. He had seen the late Sir James Simpson cut very largely into such tumours without causing much bleeding.

ON THE MODE OF INTRODUCING THE MIDWIFERY FORCEPS, WITH NOTE ON THEIR MODIFICATION.

By James Cappie, M.D.

May 10, 1871.

I have no intention to furnish a complete essay on the midwifery forceps. I shall not at present discuss the various interesting points that bear on the questions as to when to use them, or how to work with them when applied. I restrict myself to a single point in their management; and one in regard to which, if we were to judge from the uniform instructions given in manuals of midwifery, it might be supposed there could be very little opportunity to suggest anything novel. In the directions for introducing the blades, an almost stereotyped phraseology has been used by one writer after another; and as the practice I have been in the habit of following is in some particulars different from these directions, it may not be useless to bring the subject before this Society. I shall not be surprised, however, if I find that the plan I have now to suggest is one very frequently followed in practice, although not even alluded to in the ordinary text-books.

The instruction I have to criticise is, that in introducing either of the blades, it is to be held lightly in the right hand, and that two or more fingers of the left, introduced

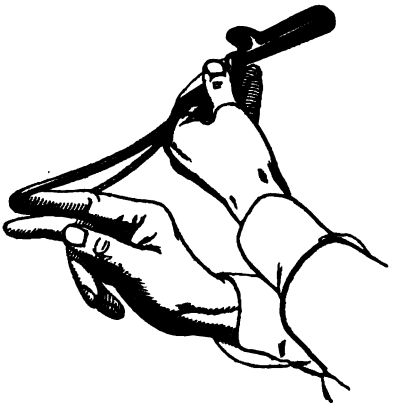
into the vagina, should guide the point of the instrument, and guard the parts of the mother; and the amendment I have to submit is, that the task of introducing, guarding, and directing should be trusted entirely to the fingers of the right hand; and that, in the first stage of introduction, the left, crossed over the right wrist, should have little more to do than to support the shank or handle of the instrument. The advantages of this plan are, that the ordinary obstetric position does not require to be disturbed,—that in many cases the instrument can be used positively with greater facility,—and, at all events, that in this way it can be applied in instances where it would be almost impossible in the ordinary way.

At the outset, I must mention that my remarks are intended to apply to the double-curved instrument. My experience of the straight forceps has been limited, and, so far as it goes, decidedly unfavourable. In leverage power, and in the thorough command we have over it, it is greatly inferior to the long forceps.

The first objection I have to the ordinary instructions is the awkward position in which we are directed to place the patient. She is to lie as much as possible right across the bed; and, according to some writers, the nates are to be made to project over its edge. To put her in this position of course involves trouble to the patient, and all the anxiety to her and her friends that attends preparations for what is regarded as a serious operation. It will not be denied that, when it has been determined to use instruments, the less formidable the procedure can be made to appear to the patient and her friends the better. Other things being equal, that plan should be adopted which can be gone about in the quietest manner. The position across the bed, however, is quite necessary if two or more fingers of the left hand are to be introduced into the vagina. When the perinæum is relaxed the ostium vaginæ dilates, not from before backwards, but more nearly in the direction of a line drawn in continuation of the symphysis pubis. The twisting of the left wrist,

therefore, would be extremely awkward if the body of the patient is not kept well forward.

With the plan I have followed, and would now advocate, the ordinary position does not require to be interfered with. The patient simply requires to be conveniently near the side of the bed. In very many cases it is a matter of indifference to myself whether she be lying on her back or on her side. If she is not under chloroform, and is anxious to facilitate procedure, I prefer a posture between the two. The right thigh can thus be more readily managed than when she is lying fairly on her side.



In regard to immediate management, we may take one of the simplest cases, where the head is at the outlet and the perinæum is relaxed. An examination being made in the ordinary way with the right hand, the blade to be applied first—that is, in most cases, the lower or left iliac blade—is held in the left hand (which, as I have already said, is crossed over the right wrist), and the point directed to the margin of the outlet. If the perinæum be well relaxed it will at once slide within the vagina, along the side or inner surface of the finger. Or, allowing the middle finger to remain within or at the edge of the vagina, the forefinger is made to catch on the inner rim at the rounded end of the fenestrum, and to draw the point cautiously onwards. If the patient be stout and the perinæum thick, a little care is necessary to prevent any fold of skin or mucous membrane being caught by the edge of the instrument. One or other of the fingers feels cautiously round while the point is being coaxed onwards; for, of course, through all the stages of introduction and

adjustment any great amount of force is out of the question.

The more firm the perinæum is, the more necessary it is for the handle of the instrument to be directed well forwards. If the outlet of the pelvis be contracted, the handle may with advantage be even inclined to some extent across the left thigh, so that it is made to describe a complete half-circle as the blade is being made to sweep round into its proper position on the head.

When the point of the instrument is fairly within the vagina, its progress and direction are at first to be trusted almost entirely *to the forefinger pushing on the rim of the fenestrum.* I believe those who have never tried this plan will be surprised to find how easily it can be urged onwards, and how safely the proper direction can be given.

Of course, the direction for the first blade is, as a rule, first backwards along the perinæum towards the sacrum, and then downwards and forwards, so that the outer surface of the instrument is toward the left ilium; and for the second blade, it is first backwards and then upwards and forwards, so as to lodge it between the child's head and the right ilium.

As long as the rounded point of the instrument is within reach it is pushed onwards by the forefinger, and the left hand has scarcely anything more to do at this stage than to allow the handle to rotate, and to be elevated or depressed according to circumstances. When the finger has pushed it onwards as far as it can reach, the left hand becomes the propelling force. The forefinger, however, still acts at any convenient point as a fulcrum to assist in giving the proper direction to the blade, while the hand gives the rotating and forward movement. If the os uteri has previously been slipped over the child's occiput, there is usually no difficulty now in at once adjusting the blade to the proper position on the head.

In this process there is wonderfully little risk of any injury being done to either mother or child. Ramsbotham indeed remarks, that if "a circular sweep of a portion of

the pelvis is made, the maternal structures might be endangered." But it is really difficult to conceive how such an accident could happen, except by the grossest carlessness. When the perinæum is at all distended, the surface of the vagina is so smooth that it would be difficult, even intentionally, to make the edge of the instrument to catch on any fold of the mucous membrane. If the head is high in the pelvis the probability of doing so is still small, and may be avoided by the most ordinary care. I have applied the forceps in the way now described for twenty years, and have never met with any accident to the maternal passages by the sweep of the instrument. In regard to the progress of that part of the blade which is out of reach, the operator has first to have a clear conception of the direction he wishes it to take, and then to proceed with the caution, but, at the same time, with all the confidence that the surgeon has in passing the male catheter.

If the os uteri is still felt all round on the head, the *tactus eruditus* of the forefinger comes to be of great assistance. The instructions usually given to introduce the fingers of the left hand within the os, and to feel for the child's ear, appear to me to be not only needless but to be cruel to the patient, and indeed the attempt must often be difficult for the operator. The forefinger by itself, being intimate with the topography of the localities, can at once determine the position of the instrument relatively to the passages and the child's head. It not only takes up greatly less room, but it is positively more efficient. It is usually a very simple matter to introduce the point of the blade within the os, especially if the latter has been pushed low into the pelvis. When it is not easily reached, the method of procedure resembles what I have detailed for introducing the blade into the vagina. First, the finger tilts on the os, while the left hand cautiously advances the point of the blade till it rests on the child's head. Then the finger hooks itself into the fenestrum, and, if possible, keeps the point sliding

along the scalp till it catches the lip of the os and is pushed within it. The part at which I prefer to enter the instrument within the uteris is, for both blades, at the posterior or sacral portion of the os uteri.

If the point has passed the os, and we are uncertain whether it is outside or within the lip, all that is necessary is to draw back the instrument slowly while the finger is resting on the os. If it be within the lip, it will be caught on the tip of the finger, and no hesitation need then be felt to urge it steadily onwards, and to give the necessary rotatory impulse to bring it into position.

It may sometimes be an advantage to be able to apply the forceps although the os uteri is not completely dilated, as in cases of severe convulsions, when uterine action is completely in abeyance, and the only hope for the mother is in the speedy termination of the labour. Now, in the manner I have described, the forceps may be passed into the uterus when the os is scarcely more dilated than just to admit the breadth of the blade. On this point, however, I shall not at present dilate, as I have brought it before the Society on a previous occasion.*

In regard to the introduction of the second blade, the same general management is to be attended to. Introduced in front of the first blade, it is directed backwards along the perinæum, and then upwards and forwards towards the right ilium. Here, as before, if the head be low in the pelvis, no difficulty is experienced in introducing it within the vagina, and in adjusting it on the head. If the outlet is contracted, the handle may be inclined across the right thigh, at the first stage of introduction, and a complete half-circle be described in the adjustment. If the perinæum is thick, or the patient stout and with sturdy muscular thighs, a convenient mode of introducing the second blade is to project the handle so well forward that the left hand can support it in front of the thighs. When the patient is lying on her back, it can thus be passed with great facility. The forefinger again acts as the pro-

* *Edin. Med. Journal*, Oct. 1868.

pelling agent in the first stage; and, according to circumstances, it does so either behind or before the shank of the first blade.

There is one point which requires a little study in the process of adjusting the second blade. Very often, when it is being made to sweep round to its position on the head, the handle is apt to be caught on the mattress; and this may sometimes be to an extent to make it appear to be impossible for the sweep to be completed. Usually a little coaxing will overcome the difficulty, but if this is not sufficient, all that is necessary is to get the patient to lie a little more on her back, and the handle may then be made to go round without interruption.

In regard to the locking of the blades I have nothing new to suggest. In most of the cases they lock readily with the first attempt. When they do not do so easily, an examination must be made to ascertain whether one of them cannot be better adjusted. I believe it will be found that if one of the blades inclines to fall easily out of position, that is the one which requires to be further introduced or rotated. It is seldom necessary altogether to withdraw either blade.

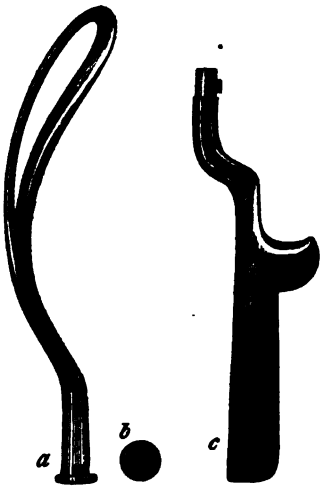
If the adjustment cannot be improved, some force may be employed to complete the act of locking. The shanks are brought into contact as near the lock as possible. Then each handle being held firmly, the one in the right and the other in the left hand, the thumbs are made to press steadily on the one side of the lock, and the forefingers firmly to support the anterior shank on the other side. The point of contact is the fulcrum, against which each handle, as a lever, is made to press from opposite directions. Considerable force can thus be applied, especially if the handles have transverse rests below the lock. Of course, the hands are ready to complete the locking as soon as the twisting or compression is sufficiently accomplished.

This paper might be considerably extended by considering what may be done in particular contingencies,

but this is unnecessary. I wish my remarks to be taken as hints rather than elaborate details. I believe, indeed, it would be impossible to lay down rules that must apply to all the cases that one meets with in practice. A great deal must be left to the common sense of the practitioner in adapting general instructions to the peculiarities of individual cases. Where there is pelvic deformity, or malposition of the head, or strongly developed pelvic ligaments, more depends on the mode of traction and general management than on the mere act of introducing the blades; but these are points on which I do not enter in the present communication.

Note on Modification of the Midwifery Forceps.

A few years ago * I showed an instrument at this Society, intended to be such a modification of the long forceps, that the shank being cut at the middle, a joint was contrived by which the pieces could with facility be joined or detached. The Simpson forceps could thus be made as portable as the common short forceps. In contriving the joint, there was a rather intricate mechanism of spring and catch, to prevent the pieces becoming too readily disjoined. After some experience, however, I have discarded this mechanism as quite unnecessary, and for several years have found the simple instrument, which I now show to the Society, to answer admirably.



The two portions are joined by what is known as the bayonet joint. On the blade portion, *a*, there is a socket, in the rim of which is a notch, *b*, which leads to a horizontal

* *Edin. Med. Journal*, Dec. 1862.

slit extending about one-third round the circumference of the socket. On the handle portion, *c*, is a nipple which fits in the socket, and on it there is a small knob which enters at the notch. When the nipple is fairly inserted and a quarter turn is given to the handle, the knob slides along the horizontal slit, and the blade and handle portions have then exactly the same relations to one another that they have in the solid instrument.

In making the socket, it is very necessary to attend to the direction of the slit. In both blades the notch for receiving the knob of the nipple is opposite the concave *edge* of the blade, and the slit itself terminates opposite the concave *side*. It will then be found that, with either blade, the rotatory necessary to join the pieces is in the same direction as the rotatory movement that must be given when the blade is to be adjusted to its position on the head.

On looking at the instrument as held in the hand, I daresay the first impression may be that the joints are insecure, as the tips of the blades appear to fall readily inwards. But it is to be remembered that in practical use the child's head is between the blades, and when they are locked, the whole instrument is as immobile as the solid-stemmed long forceps. It is then an impossibility for either handle to slip away from its blade portion. The whole of the strain in compression and traction is thrown on the solid part of the socket.

In using the instrument, I first introduce and adjust the blades, and then join the handles. Sometimes, however, as when the head is at the brim of the pelvis, I may join the handle on the second or upper blade, to assist in the rotation necessary to place it in position on the head. I do not find that the blade, when once adjusted, tends to slip out of its place more readily than that of the ordinary instrument. After introducing the first blade, I have purposely left it alone for some minutes without finding it displaced. Indeed, one would naturally expect

that the absence of the weight of the handle would be rather an advantage in this respect.

The act of joining the handles is a very simple process. The notch in the rim of the socket being felt for, the nipple is introduced with the knob opposite the notch. When fairly inserted, the handle of the lower blade is turned upwards, and that of the upper blade is turned downwards. The instrument now resembles the ordinary forceps, and locking is made in the usual way; of course, care is to be taken that the shank of the upper blade is in front of that of the lower when the handles are joined.

As, in my former paper, I narrated some cases to illustrate the advantages of this modification of the midwifery forceps, I shall not now detain the Society by further remarks. I may only mention that, after other nine years' practice, I have never experienced any serious difficulty in regard to the speculative objections that were urged against the instrument.

Professor Simpson said he had been in the habit of using the right hand as a guide in introducing the forceps, but not in the way described by Dr Cappie. He thought it a good plan to direct the point of the blade more towards the hollow of the sacrum than was usually done. It was a matter of indifference how the patient was placed, if chloroform is used. Sometimes with the patient on the back, the operator can exert more purchase and power.

Dr Gordon thought that it was an excellent plan to pass the blade on with the forefinger inside the fenestrum.

Dr Bruce said he was in the habit of keeping the patient in the ordinary position. He used the left hand to hold the instrument, but twisted the wrist forwards. He found it a good plan, too, in introducing the first blade, to push it well downwards and backwards. Sometimes with the patient lying on her back more power could be used.

Dr Duncan said he never found any difficulty in applying the instruments. They might be applied in almost any

way, but the difficulty usually came after. It would not do in all cases to apply the blades along the curve of the sacrum. He thought that in all cases the pelvis of the patient ought to be close to the edge of the bed. It was no doubt desirable that the operation should be made as little formidable as possible, but the patient should be placed in the best possible position.

Dr Keiller said that his views corresponded with those of Dr Duncan. He was of opinion that it would not do to teach young and inexperienced practitioners that Dr Cappie's mode of introducing the forceps was the proper one. He always liked to have the patient placed well across the bed; and it is often necessary to use more than the forefinger to ascertain with certainty where the point of the blade is going. When there is much doubt or difficulty, he advises his students to introduce the whole hand into the passages. It has been suggested that the handles of the instrument might be turned backwards, so as to make the operator independent of the position of the patient. He often thought that instruments might be made without handles, their place being supplied by the fingers.

Dr Young agreed with Dr Keiller as to the propriety of introducing the whole hand in cases of doubt.

Dr Cappie made a few remarks in reply to the observations on his communication.

CASE OF ADHERENT PLACENTA.

By Alexander Keiller, M.D.

May 24, 1871.

Dr Keiller exhibited a placenta with clots which he had removed from a patient on Monday morning last. He found that the placenta was not separated, and on introducing fingers to hook it down he found it was adherent, and required to be detached. On its removal, a small portion was still left adherent, and on extracting the membranes he found that portion attached to them. The uterus did not contract satisfactorily, which might have been due to

chloroform, which had a marked effect on the strength of the pains. Ergot was given, but the patient lost a good deal of blood and ran a great risk.

Dr Cairns said the case reminded him of one he had lately been called to. The patient had been attended by a midwife, who stated that the afterbirth was all right, but subsequently severe hæmorrhage occurred, which was only stopped by the removal, several days after, of a portion of the placenta which had been left in the uterus and was protruding through the os.

Dr Bell agreed with Dr Keiller in the propriety of pushing the finger through the root of cord to assist in the removal of the placenta, when the cord was weak. He had often adopted this plan with success.

Dr Sidey said that he had recently had a case of adherent placenta, and referred to sacculated contraction of the uterus, which was generally found at the upper and back part of the womb.

Dr Keiller also mentioned a case he had had of large flattened adherent placenta, without hæmorrhage.

Mr Pridie alluded to hour-glass contraction as a cause of retention of the placenta and hæmorrhage.

Dr Pattison said he did not believe in hour-glass contraction. In his younger days he thought he had met with it, but later experience had changed his views.

Dr Keiller could not see why we should not have so-called hour-glass contraction,—the muscular fibres so acting as to cause contraction of the uterus and retention of the placenta,—but he thought that the name was unfortunate.

Dr Bell said his difficulty was to understand how, in a uterus 16 inches in circumference, a contraction should occur which would prevent the passage of the finger. He thought it was a wise provision that the cervix contracted even before the placenta was expelled; its retention in this way might be caused, but he could not understand how such a contraction in the body of the organ could take place, as the arrangement of the uterine fibres did not explain it, they being oblique, whereas in the cervix they were circular. There might be irregular

contraction of the fibres on one side enclosing the placenta, but he doubted the occurrence of hour-glass contraction as it was generally described.

Dr Keiller observed that, while it was true that the fibres of the cervix were circular, the fibres of the fundus interlaced in all directions, and hence by their contraction, after the removal of the placenta, hæmorrhage was prevented.

Dr Ritchie had recently had a case of adherent placenta, and in attempting to separate it, a portion, as in the case before the Society, remained in the uterus. It was separated with some difficulty; the patient lost a good deal of blood, but made a good recovery.

CASE OF SHORT CORD.

By A. M. T. Rattray, M.D.

May 24, 1871.

Dr Rattray said that he expected to have been able to show to the Society a placenta, the cord of which measured only 10½ inches, but it was too much decomposed to bring with him. In a previous labour, the cord was found to be 13½ inches long. The labours were natural.

AN INSTRUMENTAL LABOUR, COMPLICATED WITH ADHERENT PLACENTA AND HÆMORRHAGE.

By Clarkson Cuthbert, M.D.

May 24, 1871.

About 9 P.M., on the 16th April, I was summoned to see Mrs W., who was in labour with her ninth child. I found her in bed. Pains at short intervals and weak; os fully dilated; membranes unruptured, and the head above the brim. I also found the antero-posterior diameter diminished in consequence of a flattening of the pubes.

Progress of labour very slow. I ruptured the membranes, and at eleven o'clock gave ergot, which, in about thirty minutes, improved the pains.

In her last confinement, Mrs W. was delivered of a very large child by the help of the forceps; and as she had suffered a great deal on that occasion, she besought me to use them now and relieve her. The child in the present labour was also very large; and I found that the head was making no progress, but remained immovable on the crest of the pubes. The labia and the soft parts generally were very much swollen and congested.

Being of opinion that it was necessary to use forceps for my patient's relief and safety, I introduced them about twelve o'clock. They were easily applied. When I got the head as low down as the perinæum, I took them off. The head was born with some difficulty, but when the shoulders took up their position at the outlet, I had the greatest difficulty in getting them through. The right shoulder got hitched above the pubes in the same way as the head, and, owing to the extreme breadth of the chest, they were firmly grasped by the surrounding parts. I could scarcely get down an arm to release the infant. At length I managed to get the right arm born, which relieved the constriction, and the labour was soon completed. The child gasped several times before respiration was fully established, and, before securing the cord, I allowed a drachm or two of blood to ooze away, which relieved the infant considerably. The child was very large. After waiting half an hour, as the placenta did not come away, I introduced my left hand, and found it adherent on the posterior wall of the uterus. It was carefully peeled off, and the whole mass was brought away. About a third of its surface was adherent. On withdrawing the placenta, a pretty smart flooding took place. I immediately gave a full dose of ergot, grasped the fundus firmly with my left hand, and applied cold to the vagina and over the region of the uterus. This was persevered in till all hæmorrhage ceased; after which the

soiled linens were taken away, and a binder with a compress applied. In about an hour after delivery a violent shivering, with quick gasping respiration, came on, and the patient complained of cold coming on suddenly, and extending along her legs, thighs, and body. On applying my hand over her knees and face, I felt them ice-cold. The pulse at the same time became quick and feeble, but there was no hæmorrhage. I gave her a wineglassful of whisky, which was repeated in fifteen minutes. A bottle of hot water and a hot brick were applied to the feet and legs; friction applied to the limbs, and more bedclothes heaped on. In a short time reaction set in, the pulse improved, and in three-quarters of an hour the attack had passed away. I left about 3 A.M., leaving my patient quite comfortable. Mrs W. made an uninterrupted recovery.

The following are the child's weight and measurements at birth:—Weight, 12 pounds; length 22½ inches; circumference of the shoulders, 18¼ inches; circumference of the hips, 14½ inches.

Remarks.—This case illustrates very well the value of the forceps applied early. There is no doubt that the flattening of the pubes and the great size of the child were the chief causes of the difficulty. The head lay, as it were, hitched on the pubes, and from this position it would not move. The right shoulder was caught in the same way, and this, of course, was another source of difficulty. The swelling and congestion of the labia and surrounding parts, caused by pressure, also demanded immediate relief.

The shivering, with extreme cold, no doubt was caused by the shock to the nervous system. After tedious and complicated labours, the collapse is sometimes very great, and may even lead to fatal consequences if the patient does not speedily rally.

I have stated that my patient, in a previous confinement, was delivered by the aid of instruments. On that occasion, I believe there was also adherent placenta, fol-

lowed by severe hæmorrhage. A sketch of the history of the case has already been given to the Society.

In conclusion, allow me to remark, that it is easier to remove an adherent placenta with the left than with the right hand, especially when it adheres to the posterior wall of the uterus. This will appear more evident, if we remember that, as the patient lies usually on her left side, the axes of the pelvis and uterus are parallel to the bend of the left arm. I would therefore recommend practitioners, especially the junior members of the profession, to accustom themselves to manipulate with the left hand in cases of adherent placentæ or in turning.

Dr Keiller remarked, that hitching of the head or shoulder on the pubes was a not uncommon cause of difficult labour. The shape of the woman had much to do with its occurrence, in cases, for instance, where the curves of the spine were very marked. It is important in such cases that the uterus should be supported to allow the head to enter the pelvis in proper position.

Dr Bell said that, in introducing the hand to remove the placenta, much would depend upon its position. If placed posteriorly, the left hand would be used more easily, and if anteriorly or laterally, the right.

ON POST-PARTUM DIETETIC TREATMENT.

By Thomas Cairns, M.D.

May 24, 1871.

A few days ago, I was asked by the Senior Secretary of this Society if I could furnish a short paper for this meeting, the reason being, I presume—although none was assigned—a dearth of matter. Having the interests of the Society deeply at heart, and bearing in mind that it professes to deal rather with practical than theoretical subjects, the after dietetic treatment of puerperal women immediately suggested itself to my mind as a subject of

prime importance, and one likely to give rise to a considerable amount of interesting discussion between the elder and younger Members of the Society. I accordingly selected that subject, and duly intimated to Dr Ritchie that I would do my best either to read or transmit a short paper upon it to the meeting of this evening. That paper I now respectfully submit for your consideration. But for a recent enactment, whereby the Society restricts Members to certain limits in the reading of papers which they produce, I would have discussed the whole subject of post-partum treatment, including as well the use of binders, and the use of purgative medicines, as articles of diet. Your new regulations, however, prevent this, and therefore I shall confine myself *almost entirely* to the simple treatment of diet, reserving the other parts for a future occasion.

To come now to the point under discussion, viz.:—"What is the *correct* and *common-sense* treatment of women in point of *diet after labour?*" The general answer to that question is this,—"Every parturient woman requires a dietetic regimen suited to her particular state and condition." That is to say, no absolute rule can be laid down applicable in all circumstances to all women without exception. For example, a woman after her confinement may not only have no desire for food, but a positive loathing of it, and to force her to take it in such circumstances would only be productive of harm instead of benefit, inducing in all probability acidity of the stomach, flatulence, and diarrhœa. Again, some women have very marked idiosyncrasies in reference to certain articles of diet. Some cannot eat an egg, others cannot eat beef, others mutton, etc., without producing very serious symptoms. While, therefore, I acknowledge that it were rash in the extreme to lay down any one invariable and fixed rule as to what kinds of food should be administered in puerperal cases, I humbly venture to allege,—Primo, *That the diet should be nutritious in point of quality.*

For several years past I have been in the custom of asking all my new patients, both in private and dispensary practice, what sort of food the "doctor" allowed them to eat after their confinement; and the invariable and uniform answer I have received to that question has been as follows:—"Oh, doctor, just the usual things." "And what things, may I ask, are these?" "Oh, just breadberry and water-gruel." This forenoon I asked a patient who has just come from the country—where she had been delivered of ten children, and who has now been delivered of her eleventh, still-born, owing to shoulder presentation, age of patient, and length of time allowed to elapse before medical assistance was called in—the very same question, which was followed by the usual answer, with this addition, that she was kept under that treatment for *ten* days during *all* her confinements; whereas, in most other cases I have found it limited to the fifth or sixth day. Such treatment, I humbly maintain, is utterly inconsistent with the principles of obstetrical pathology. I wish I had time to inform the younger Members of the Society the real meaning of the term "Labour," because I am certain if they knew what it actually expresses, they would at once perceive the necessity, on purely pathological grounds, of *feeding* and not *starving* their puerperal patients. The word "labour," however, in our own language, sufficiently expresses what a woman has to suffer in the act of parturition. During a pain the acceleration of the pulse, the profuse perspiration, the contorted features, and the agonizing cries, etc., of the patient are quite enough to convince even the veriest novice that she is *labouring* in a *manner* in which no *man*, however hard his struggles, *ever did* or *could* suffer as she does. This being so, consider the *waste* of *tissue* to which she is subjected *during* her confinement, and you *must see* that, unless that waste be supplied by proper and fresh materials, the patient's strength must necessarily be very much reduced below its normal condition; and should any untoward event happen to her

in these circumstances, the prognosis must essentially be infinitely more unfavourable than if her system had been duly sustained by generous diet. On these grounds, I have always been in the custom of liberally administering to my puerperal patients the most nourishing food which their circumstances enable them to procure, such as soft boiled eggs, beef-tea, soups, chops, steaks, tripe, etc., with a glass of wine daily in addition, or, if the patients prefer it, a glass of ale or porter; and this treatment, I humbly aver, is based on sound pathological principles.

Secundo, *The diet should be small in quantity and frequently repeated.*—During labour the digestive and assimilative powers are weakened, as well as other parts of the system, and to exhibit large quantities of food in these circumstances would be to entail upon the stomach a much greater amount of work than it is able to accomplish. That organ fulfils its duties better perhaps than any other organ in the human system, and just because it seems to know that if it fail in the discharge of *its* functions, every department of the great and mysterious laboratory contained in the human frame will be thrown into confusion and disorder. But, for the very reason now mentioned, the organ alluded to should be treated with especial leniency and kindness. It is so willing to work that it should never have devolved upon it even such an amount of work as itself is willing to undertake; it is so obliging and so apt to exceed its own powers, that it should never be allowed an opportunity of doing so. Administer more than it can easily digest, and it will tax itself to the utmost to digest what remains in excess, when, after finding its efforts abortive, it either ejects the undigested articles by the mouth, or expels them by the gut, or allows them to remain and undergo fermentation—all of which is most unfair treatment to the stomach, and consequently highly prejudicial to the patient. I say, therefore, let food be administered in small quantities at a time, and at such intervals as shall have ensured the complete digestion of the previous diet.

Tertio, *The diet should be varied in kind and form.*—Monotony is death, variety life, to the human soul. The eye tires in time of the sublimest spectacles, the ear of the sweetest music, and the tongue of the most delicious morsels. Even in health, the richest viands, when continued from day to day, become unpalatable and even nauseous. A change of diet, in short, is indispensable to a proper relish for food and the maintenance of the appetite. And if so in a normal condition of health, how much more on a bed of sickness. In puerperal cases, therefore, common sense seems to suggest that, with the view of stimulating the appetite and imparting to the patient a positive relish for food, every advantage should be taken of the culinary art in dressing the same article in different forms, and when these have been exhausted, that one article should be substituted for another during the whole period of the patient's convalescence.

Dr Pattison asked when Dr Cairns began to give the nourishing diet?

Dr Cairns said, on the day of confinement.

Dr Keiller said that communications on the same subject had been read at the last meeting of the British Medical Association, and most of the speakers who took part in the discussion were in favour of the views contained in Dr Cairns's paper. The dietetic treatment should be regulated in accordance with the habits and circumstances of the patient. He thought that the sloppy treatment was fast going out.

Dr Menzies did not think the breadberry and water-gruel treatment was so much in vogue as formerly.

Dr Bell said that for thirty years he had followed one plan. Immediately after delivery, he gave a glass of whisky, brandy, or wine. In one case in which this had not been given, he had cause to regret the omission. He agreed in the propriety of keeping up the system, but much must of course depend on the habits and constitution of the patient. In most cases, he was in the habit of allowing butcher-meat and wine on third day. He referred to the case of the late Princess Charlotte, who sank from exhaustion consequent upon hæmorrhage. She had only

water-gruel—no stimulants. In Shetland, he understood that a bottle of whisky was given to the patient immediately after delivery, to be kept in bed beside her.

Dr Sidey said that the state of the tongue, rather than the length of the labour, guided him in his dietetic treatment. The tongue was often dirty, even when the labour was easy. When the tongue is clean, diet of the kind recommended by Dr Cairns might be safely given, but the same would not do for the other class.

Dr Ritchie agreed theoretically with Dr Cairns's recommendations, but found that practically he frequently required to give a less nourishing diet than he wished, especially in those cases in which the tongue was foul, and also when the patient was not inclined to take animal food.

URETHRAL CALCULUS.

EXHIBITED BY A. M. T. RATTRAY, M.D.

JUNE 14, 1871.

DR RATTRAY exhibited a small calculus which he had extracted from the urethra of a little boy, who had for some time previously suffered severely from spasmodic pain.

CASES OF PHLEBITIS AND FACE PRESENTATION; WITH REMARKS ON THE EDINBURGH ROYAL MATERNITY HOSPITAL.

BY CHARLES BELL, M.D.

JUNE 14, 1871.

So much has been said and written against lying-in hospitals recently, that I should have hesitated to bring again any cases treated in them under your notice, had I not been satisfied that the prejudices against such institutions were not well founded, and that the high rate of mortality in some of them was not owing to their inherent

nature, but to their being overcrowded and ill managed. I repeatedly expressed this opinion to the late Sir James Simpson, who admitted that it was correct. After his remarkable philippic against maternities, in which he stated that their ordinary mortality amounted in 1 in 29, I asked him if he was aware of the statistics of his own hospital? He replied that he was not; and when I told him what they were, he expressed his surprise; and, at a subsequent public meeting, he corrected his former statement by giving the correct account of that institution at that time.

Being desirous of proving the correctness of my views, I took a deep interest in the Royal Maternity, to which I had been appointed one of the Ordinary Physicians, and I resolved to avail myself of the opportunity it afforded of carrying them into effect as far as possible, and it fortunately so happened that I had almost the sole attendance of the Hospital for nearly ten months consecutively, which gave me great advantage. But although I had the concurrence of Sir James Simpson, I found it no easy task to effect its improvement, as I met with strong opposition from some of those who had the management, or rather mismanagement, of the institution.

It would be difficult to imagine any public institution more disgusting and thoroughly mismanaged than the Royal Maternity was when I joined it. The wards were filthy in the extreme. The beds were not fit for human beings to occupy; the food of the patients was so bad and ill cooked, that they frequently could not eat it. It was not remarkable, therefore, that the mortality should have been comparatively high, although it did not amount to what Sir James Simpson represented was the case in similar institutions.

Fortunately, for the sake of Edinburgh and humanity, an active and judicious house committee was appointed, and soon established some admirable regulations, which were fully carried into effect by our excellent matron, Mrs Hay, who had had great experience in the manage-

ment of such institutions; and, in consequence, a complete reformation was brought about in the appearance and comfort of the Hospital, which was soon illustrated by the improved condition of the patients, and the remarkable diminution in the mortality, both among the mothers and the children. The maternal deaths diminished from 1 in 66 to 1 in 233; while the deaths among the children fell from 1 in 5·74 to 1 in 24. These beneficial results were, in a great measure, to be accounted for by the marked attention paid to the personal cleanliness of the patients, who were bathed with a solution of Condy's fluid twice a day after their confinement; and to their number being limited according to the size of the wards, which were regularly purified and well ventilated; and to the mothers being induced to suckle their children so long as they remained in the Hospital.

These statistics form a favourable contrast, not only to what is reported of other maternities, but to the general results of private practice, which, if I mistake not, are stated by Dr Matthews Duncan to be 1 in 108. I am therefore justified in repeating the assertion, that the high rate of mortality in lying-in hospitals is not dependent on their inherent nature, but on their mismanagement. This fact appears to be strengthened by the consideration of the following table of the cases treated in the Maternity from the commencement of the regulations I have referred to, until the end of December last, being a period of nearly fifteen months:—

Number of patients, . .	223	Forceps cases, . . .	8
Primiparæ, ...	171	Turning „ . . .	2
Multiparæ, ...	42	Cephalotripsy, . . .	1
Number of operations, .	11	Deaths, ...	1*

With these preliminary observations, I shall now relate a few cases which came under my observation many years

* This occurred in a primipara, who had been in labour for five days previous to her admission to the Hospital, and exposed to frequent examinations. She had a small pelvis, and the position of the fœtal head was very obscure at first.

ago; and as they give an illustration of phlebitis, a disease of rare occurrence, except in hospital practice, I think they may be interesting to the Society.

Case of Phlebitis.—Mrs Cousins, aged 30, a large flabby woman, having a very unsettled expression of eye, was delivered of her third child, a girl, on the 20th November, after a natural and rapid labour, being at the full period of gestation. Immediately after the expulsion of the afterbirth there was considerable hæmorrhage, which continued to come away for two hours in a small stream, notwithstanding that two drachms of the ergot of rye were given, and vinegar and water were injected into the vagina.

She stated that for two months previous to her labour coming on she had severe pains every Thursday afternoon, which led her to think she was going to miscarry, until she found that they gradually went off; and this being Thursday she was under the impression the same result would take place.

22*d Nov.*—Was seized with severe rigors at 4 A.M.; pulse 100 and very intermitting; no pains. She had three grains of calomel and five of antimonial powder in hot gruel.

9 A.M.—Had slight pain on pressure above the pubes; pulse 120, more regular.

8 P.M.—Pulse again intermitting; no pain; bowels freely moved.

24*th Nov.*, 10 A.M.—Complained of severe pain in her left leg, in which she had formerly suffered from varicose veins. On examination the saphena major was found much enlarged, hard, and painful to the touch, particularly at the calf of the legs and the middle of the thigh.

Four leeches were applied to this part, and the whole limb was enveloped in a linseed poultice for some hours, and when the poultice was removed the liquor plumbi acetatis was applied. She became much exhausted towards evening, in consequence of her bowels being frequently moved.

25th Nov., 8 A.M.—The pain much increased, and the swelling had extended up the limb; pulse 120, less intermitting. Eight leeches were applied along the course of the vein, and after they came off a linseed poultice was applied, and she had ten grains of Dover's powder in warm gruel.

At 4 P.M. she was again attacked with rigors, and became quite delirious. When she spoke her tongue appeared to be too large for her mouth, and her lips were much distorted. To have six grains of calomel and four of antimonial powder in gruel.

At 10 P.M. Dr Rigby ordered her to have three grains of camphor and five of hyoscyamus.

26th Nov.—She vomited the camphor and hyoscyamus, but she had some sleep during the night, and she spoke more coherently. There was less pain in the leg, but she complained much of her wrist, which was slightly inflamed. Her eyes were much suffused; tongue was coated with a whitish, slimy fur; pulse 100, and weak; passes her stools involuntarily. To have the camphor and hyoscyamus pills every two hours.

6 P.M.—More delirious; her eyes more inflamed; bowels freely moved. She seemed to have no pain in her leg, but her wrist was acutely painful. Pulse full and soft. Dr Rigby ordered her to have half a grain of the acetate of morphia and one once of camphor mixture every two hours until sleep was procured; and to have four grains of grey powder and the same of Dover's powder at bedtime.

At 9 P.M.—Pulse 64, very weak; skin cold. She was much inclined to talk and pick the bedclothes. To have three grains of camphor; to be repeated if the pulse did not improve.

27th Nov., 2 A.M.—Has had the camphor once, and the morphia twice, and she was in a state of great excitement, requiring two nurses to hold her in bed. It became necessary to put her in a strait-waistcoat, after which she fell asleep.

At 2 P.M.—Has just awoke from a sleep of nine hours. Pulse very feeble; eyes suffused, and speaks indistinctly. To have sago and wine.

About half-an-hour after last report Dr Rigby saw her, and ordered her to have brandy and an egg beat up together, and to continue the camphor and hyoscyamus.

Dr Hugh Lee saw her about an hour after, and ordered the brandy and egg to be omitted, and that she should have twelve leeches applied to the temples. The result was, she immediately sank into a stupor, and died at 9 P.M.

2. *Case of Phlebitis.*—On 15th December Sarah Masterman, aged 23, a well-formed, florid-complexioned, unmarried woman, was delivered of her first child, a small boy, apparently in the eighth month; she having menstruated in the beginning of April. Her labour was rendered tedious from the head presenting in the fourth position.

17*th Dec.*, 10 A.M.—She has had no complaint except that she has had a cough, which came on before labour, until this morning, when she had a severe rigor, followed by pain above the pubes. On examination the uterus felt large and tender; skin hot; pulse 107, small and compressible; tongue clean and moist; bowels moved by castor-oil yesterday; lochia scanty; milk plenty.

A linseed poultice to be applied to the abdomen, and to have three grains of calomel and five of antimonial powder, to be followed in four hours by a dose of the saline mixture.

9 P.M.—She feels better, and has less pain on pressure. Perspiring freely. Bowels moved after taking three doses of the saline mixture.

18*th Dec.*, 8 A.M.—The pain in the abdomen quite gone, and she only complains of cough. She, in consequence, unfortunately got out of bed and sat for some time.

4 P.M.—Is now suffering from pain in the right groin, extending down the thigh in the course of the saphena vein, which is exceedingly tender to the touch. Twelve

leeches to be applied to the most painful part; after the leeches come off a linseed poultice to be applied to the abdomen and thigh. To have two grains of calomel and five of antimonial powder.

19*th Dec.*, 10 A.M.—She has no pain.

4 P.M.—Her cough has become very troublesome, and is accompanied with bloody expectoration. She complains of great breathlessness. Pulse 120, small and resisting; face flushed; tongue clean and moist; skin hot. To be bled to the extent of twelve ounces. To have one grain of calomel and five of antimonial powder, and in two hours after to begin to take the eighth of a grain of tartar emetic, to be repeated every half-hour, in solution.

11.36 P.M.—Perspiring profusely; has less cough and breathlessness. To continue the antimony while awake.

20*th Dec.*, 7.30 A.M.—Has passed a pretty good night, and has less difficulty in breathing. Cough less frequent. Bowels freely moved. Pulse 84, feeble, and she feels weak; tongue clean and moist; countenance pale.

11 A.M.—Bowels again moved, but she has not passed water since yesterday. The catheter was introduced, and a small quantity of high-coloured urine was drawn off. On using the catheter, the right labia was observed to be considerably swollen, and the vagina and uterus were tender to the touch. There was also slight pain on pressure along the course of the saphena vein. The vagina was washed out with hot water, and a poultice was applied to the abdomen and labia. To have a powder containing five grains of Dover's powder and two of calomel immediately, and the same in four hours. The tartar emetic to be continued every two hours. A blister to be applied to the chest.

4.30 P.M.—Complains of great pain on the right side of the abdomen, immediately above the ilium, with considerable tenderness on pressure. She has less uneasiness in the chest, and she breathes more freely, and her cough is less troublesome. Tongue clean; pulse 120, rather sharp. To have the vagina washed out with a decoction of

camomile flowers. A mustard poultice to be applied to the abdomen, and the part afterwards to be rubbed with mercurial ointment, and then to have a linseed poultice applied. To have two grains calomel every four hours, and the solution of tartar emetic every hour.

8.30 P.M.—In a moderate perspiration. Has less pain in the abdomen, which she can allow to be pressed. Pulse 128. To have the vagina washed out with a decoction of poppy-heads. The medicine to be continued.

21*st Dec.*—As her bowels were much relaxed, she had five grains of Dover's powder early in the morning. In the course of the day she had four grains of calomel in divided doses. She was quite free from pain and cough, and she had passed urine freely. After taking Dover's powder her bowels were not moved until the evening, when they acted once. To have the abdomen again rubbed with mercurial ointment and the other treatment continued. To have arrowroot for supper.

22*d Dec.*—Her gums slightly affected with mercury, but has no complaint. Bowels have been four times moved during the night, and she is weak in consequence. To have five grains of Dover's powder and four of grey powder, and arrowroot for diet.

She rapidly improved, and left the hospital in good health on 31st December.

An Anomalous Case.—Jane Crow, aged 19, a thin, delicate-looking girl, with a very sallow complexion, was delivered of her first child at the full period of gestation on the 1st January, after a comparative easy labour, the head being in the first position.

On the 3d day of her confinement she had a slight rigor, followed by severe headache, which was relieved after taking a dose of castor oil. She had no other marked symptom, except that her pulse kept above a hundred, and she spoke in a hurried and unnatural manner. When asked how she was, she always said she was quite well.

On the 10th day she got out of bed and went into the convalescent ward; but she soon became weak, although

she made no complaint, and she seemed to have no pain.

On the 12th day her left arm became inflamed above the wrist, and she soon became quite incoherent, and she died on the 14th day. The friends would not allow an examination.

Face Presentation.—Mrs Cochrane, aged 19, a strong, healthy-looking woman, residing in Blackfriar's Wynd, was seized with severe pains on the 12th January, and being at the full period of gestation, she was induced to believe that her labour was coming on, and sent for one of the pupils of the Royal Maternity, who finding that the pains were spurious, left her, with instructions to send for him when the true pains came on. He did not return until the following evening, when the pains were lingering and ineffectual. He, however, remained with her for some hours, and then sent for one of the house surgeons, under the impression that it was a breech presentation, and that the first stage of labour was completed. But when the house surgeon arrived he ascertained that the head presented, and that labour had just commenced. The error in diagnosis arose from the womb being much anteverted and the head being felt through its anterior walls, and mistaken for the breech. He gave the patient thirty drops of laudanum, which allayed the spurious pains. True labour pains did not come on until the evening of the 15th, when it was discovered that it was a face presentation, and the forehead soon became impacted in the arch of the pubes, which was narrow and angular like that of the male.

Such was the state of matters when I arrived, between five and six o'clock on the morning of the 16th. I immediately recommended the application of the forceps; and the patient being put under chloroform and her urine drawn off, the house surgeon endeavoured to apply them, but without success. I then introduced them, but they slipped—an accident which had never occurred to me before, nor has it ever done so since. It arose from my

being thoroughly exhausted from having a severe and arduous instrumental case only a few hours before, and being at the time suffering from illness. I again introduced the instrument, and after great difficulty delivered the child. The skin under its right ear was lacerated, and did not heal kindly.

Having waited for twenty minutes in hopes that the afterbirth would be expelled, and feeling the womb considerably contracted, I recommended the pupil to examine if it were not in the vagina, when he found it was still in the uterus. After waiting some time longer, I examined and found the os uteri closely contracted round the cord, so that it was impossible to reach the placenta, which was in the uterus. With much difficulty the contraction was at last overcome by gradually insinuating the hand, in the form of a cone, through the os, when the placenta was found adhering throughout its whole extent, and the adhesion was so strong that it was necessary to tear it off piecemeal; and, notwithstanding my utmost efforts, I was obliged to leave a large portion still adhering, which was expelled without hæmorrhage on the fourth day.

The patient did well for twenty-four hours, when rigors came on, and they were soon followed by pain in the abdomen and headache, and the pulse rose to 125. These symptoms continued, along with incontinence of urine, for some days; but they ultimately yielded to the use of five-grain doses of Dover's powder every six hours, and mild aperients, so that in three weeks the patient was able to get out of bed, and soon after resumed her employment of itinerant dancing girl, travelling about the country with her husband, who acted as her musician. There was slight laceration of the mucous membrane of the vagina, but the perinæum remained entire. The child died on the ninth day, the laceration in its neck never having healed.

This case appears to me to be interesting from its unusual complications, and the great difficulty there was in delivering the child, in consequence of the peculiar

deformity of the pelvis. It also shows that in some cases considerable portions of the placenta may be left adherent with impunity; at the same time, this ought never to be done if it can possibly be avoided. In this case it was quite unavoidable from the firm adhesion, which could not be overcome without the risk of injuring the womb. I therefore incurred the risk of hæmorrhage, at the same time instructing the student in charge of the case to watch the case carefully.

Dr Cochrane remarked that in cases where there was difficulty in introducing the hand, he found that an opiate was of great use. He had never found it necessary to leave any portions of the placenta in the uterus during the course of a long practice.

Dr Gordon thought the uterus could be more readily acted on by chloroform, and that the contraction would be overcome more easily by it than by opium.

Dr Thomson said that eighteen months ago he had a very interesting case of the seventh month. The placenta was retained by strong contraction of the uterus. An opiate had been given more than once in the progress of the case. The contraction gave way readily when the patient was put under chloroform. He had several times been unfortunate enough as to leave pieces of the placenta in the uterus. He had once met with a case of hour-glass contraction, which prevented the placenta being removed for a long time. It was so adherent that it could only be taken away bit by bit. The hæmorrhage was very great.

Dr Gordon said that in some cases it was occasionally necessary to leave portions of the placenta, and quoted the advice of an old Edinburgh teacher,—"That we should separate all that was separable."

Professor Simpson thought that in those cases where there was closely adherent placenta, there was less danger than when a portion separated in the womb, for it was still in a vital state whilst adherent.

Dr Burn thought that portions of the placenta which were left in the uterus, and which were not afterwards accounted for, came away when the patient was at stool. He narrated a case of adherent placenta where the patient

took phlebitis with protrusion of eyeball, and died. He had also seen cases where no injury resulted from portions of placenta being left.

Dr Bell, in his reply to the observations made on his paper, stated that, on post-mortem examination, he had very often found the uterine veins filled with pus.

EPITHELIOMATOUS MASS.

Exhibited by Professor Simpson.

June 14, 1871.

Professor Simpson showed an epitheliomatous mass which had been removed from the cervix uteri of a patient, with the following history, which had been drawn up from the Infirmary journal:—

"H. B., aged 40, was admitted Ward XIV., Royal Infirmary, 22d May 1871.

"*Previous History.*—Began to menstruate at 15. Married when 19 years old. She has had seven children at various intervals. Has never miscarried. Her labours have always been easy—natural, and at full term. Six weeks after her sixth confinement, she suffered from inflammation of kidneys and general anasarca; from this she made a complete recovery within three weeks. With this single exception, she has enjoyed perfect health during all the twenty years of her married life.

"Her last confinement took place on 29th December 1870 (five months ago). She made a perfect recovery, and in a fortnight was able to attend to her ordinary duties.

"About six weeks after, she began to complain of a dragging, uneasy pain in the left side, accompanied by a thin glairy discharge from vagina, having a most disagreeable smell. She presumed she suffered from an attack of 'whites,' and paid no attention. The discharge became, however, much thicker in consistence, and enormously

increased in quantity, the smell at the same time being much more offensive. She suffered from constant dyspepsia and vomiting. Occasionally she observed that the discharges from the vagina assumed a brownish colour, and at intervals of ten days she had a sanguineous discharge lasting from one to three days. She consulted Dr Robertson, of Bannockburn, who sent her to the Royal Infirmary, to be treated by Professor Simpson.

"*Examination on admission.*—Patient is somewhat over middle height, well made, slightly anæmic. On examination, heart and lungs are found normal, complains of great weakness and constant pain extending from left groin to lower margin of ribs; appetite bad; bowels very constipated. Has nursed her infant regularly. Has a constant muco-purulent, exceedingly offensive discharge from vagina. On digital examination per vaginam, a hard, nodulated, irregular mass was found attached to the cervix uteri, encircling the whole os uteri, except at one point, and separate from the roof of the vagina by a narrow margin of healthy mucous membrane. The tumour felt about the size of an orange. Patient complained of much pain during examination. On withdrawing finger it was found covered with blood. On introducing a Sims' speculum, vagina is red and congested, covered with a yellow fœtid discharge, and at intervals streaked with blood. Tumour already mentioned is found to be of a bright red colour, irregular surface, small unequal lobules projecting here and there. The introduction of speculum was accompanied by a sanguinary discharge.

"Patient ordered Tinct. ferri muriat., and a solution of permanganate of potass to be injected night and morning. Bowels to be acted on freely. Nursing to be discontinued, and extract of belladonna dissolved in glycerine to be applied to the breasts.

"*30th May.*—The milk has entirely disappeared from the breasts.

"To-day, patient being placed in lithotomy position

and anæsthetized, Professor Simpson, by means of a wire écraseur, removed the tumour. There was a smart hæmorrhage, which was at once checked by means of pledgets of lint soaked in a solution of perchloride of iron in glycerine. *Vespere.*—Pulse 100. Patient dozing. Complains of no pain.

"31*st May.*—The plug of lint removed this morning. No hæmorrhage; pulse 95. Took a good breakfast; feels well. Vagina to be syringed with Condy's fluid and water.

"Patient continued to progress favourably until 5th June (seven days after operation).

"5*th June.*—After rising to stool, she had a severe rigor. In evening her pulse 130, full; face flushed; skin hot; much thirst. Has had two attacks of vomiting; complaining of much pain and tenderness over abdomen. Is unable to pass urine, which had to be drawn off with a catheter. Ordered ¼ gr. morphia every three hours; turpentine enema and turpentine stupes over abdomen.

"6*th June.*—Pulse 140; pains over abdomen still acute. Abdomen to be blistered with liquor vesicatorius; morphia pills to be continued. Can pass water without catheter. *Vespere.*—Pain somewhat less over abdomen; pulse 120. Has slept for three hours; feels much refreshed, and easier.

"7*th June.*—Pulse 98; pain in abdomen much relieved; passed a good night."

Dr Simpson examined the patient to-day, 14th June, fifteen days after the operation, and found the surface of the wound granulating. As there were some suspiciously prominent points scattered through it, he had brushed it freely with solution of perchloride of iron. Her general condition was decidedly improved, and the arrest of the discharge from the fungating mass, even though it might prove to be but temporary, he regarded as an immediately beneficial result of the operation. In such cases he had frequently removed portions of cancroid tumours from

the cervix uteri, where he knew that the whole affected portion of the organ could not be extirpated, with the happy result of arresting for a time the progress of the disease and checking the exhausting discharges, which must otherwise have speedily destroyed the patient's health. Sir James Simpson used occasionally to dig out with his nail the cancerous masses in some of the excavating forms of malignant disease of the cervix uteri; and Professor Simon had recently written a paper, relating some interesting cases where he had followed a corresponding practice of digging out the morbid deposits with a scoop or curette. The result was that a healthy granulating sore was substituted for a bleeding cancerous surface; and though the disease returned eventually, yet the benefit for the time to the patient's health was very marked, and her life was prolonged perhaps for months.

Dr Thomson thought the removal of such a mass must be very important, and fully warranted, as making the patient more comfortable even for a time. In a recent case of cauliflower excrescence which he had seen, it was too late to remove the whole, but by picking away portions by the nail, and applying perchloride of iron, the hæmorrhage, which previously had been great, was stopped even till time of death.

Dr Gordon submitted that his instrument for rupturing the membrane, if modified at the point, would be better suited to remove portions of tumour than the nail.

Dr Burn said Sir James Simpson had a small instrument which he used for scooping off portions when removal of the tumour could not be effected. When the hæmorrhage was from vegetations, he had frequently scraped them off with success.

Dr Young also took part in the discussion.

Professor Simpson replied.

SPECIMEN OF HYDATIGINOUS OVUM.

EXHIBITED BY A. M. T. RATTRAY, M.D.

JUNE 28, 1871.

DR RATTRAY exhibited a specimen of a large hydatid mass, which had been expelled after the administration of ergot in drachm doses. Three doses were given with an interval of half-an-hour between each dose.

The patient, a lady of 33, has had three children. Dr Rattray was called in on 26th June, and found her pale, anæmic, and greatly reduced from hæmorrhage.

The case was rather difficult of diagnosis, but was treated as a threatened miscarriage by Gtt. vi. Tinct. Cannabis Indicæ, thrice daily, and cold water enemata. On 27th, severe and labour-like pains supervened, and in a short time a large mass of hydatids was expelled, and what appeared to be part of a placenta remained adherent to the uterus. The patient became very weak through loss of blood; and, taking a serious view of the case, her husband, at my request, soon brought Dr Keiller to my assistance. He removed the adherent mass, she having been previously put thoroughly under the influence of chloroform. Recovery was speedy and good.

Four things are worth mentioning regarding this case:—

1. The patient considered herself four and a half months advanced in pregnancy.

2. She noticed that there existed a greater enlargement of the abdomen at the same period of gestation than in any of her former pregnancies.

3. Sickness continued both day and night for fully three months; whereas, in previous pregnancies, vomiting commenced as soon as she became *enciente*, and continued for the first month, but only during the day.

4. In addition to severe headaches, she felt for several weeks as if a great oppression or weight had been laid on the top of the head.

Dr Keiller said when he saw the patient, she was in a dangerous state. He examined under chloroform in usual position, the hips well over the edge of bed, and one hand making pressure over the uterus, whilst the whole of the other was introduced into the vagina, and two fingers into uterus. He found a mass of this placenta-like substance, which he removed by separating it from the uterus. He thought the mass was entire, but it came away in three portions. There was little hæmorrhage at that time. All the structure was removed. He thought it very injudicious to leave any portion of placenta, if it can be helped. He alluded to the difference of cases in abortion. In natural abortion the ovum is dead, and nature expels the mass. In criminal cases the parts are vital, and take time to separate, and from this reason, he considered criminal abortion was so frequently injurious.

Dr Cochrane concurred in Dr Keiller's remarks on danger of retention of placenta.

Dr Bell said such cases are sometimes attended with all the bad consequences of labour at the full time. He alluded to a case in which the patient fevered and died after the expulsion of hydatid mass.

CASE OF RETAINED PLACENTA.

EXHIBITED BY ALEXANDER KEILLER, M.D.

JUNE 28, 1871.

DR KEILLER exhibited a placenta which had been left in the uterus for some days, after what should have been the termination of a case. The patient was attended by a student, who said she had aborted, but hæmorrhage still continued. The woman, when Dr Keiller saw her, looked as if she was dying. He introduced his hand in the same way as in last case, although he thought she was past recovery. He found the mass projecting from the cervix uteri, and removed it. Ergotine had previously been injected under the skin to check hæmorrhage. Brandy and ergot by mouth were subsequently given.

She rallied and did well. He did not remember having seen a woman so far gone from hæmorrhage, she being pulseless, recover. He alluded to another case, in which he suspected abortion had been attempted. The placenta was not expelled at the time, and she took pyæmia and died. Had the placenta been removed at the proper time, he thought she might have recovered. He also made remarks on other cases illustrative of the subject.

CASE OF CEPHALOTRIPSY.

By Alexander Keiller, M.D.

June 28, 1871.

Janet H., æt. 21, was admitted to the Maternity Hospital on the evening of the 19th June 1871. She was found to be in labour with the head presenting and prolapse of the cord. The antero-posterior diameter of the pelvis being found to be diminished to something under two inches. Dr Keiller was sent for at once. In the meantime attempts were made to replace the cord, but were unsuccessful. On Dr Keiller's arrival, he at once decided, after examination, on performing cephalotripsy. The head having been punctured, the cephalotribe was introduced and the head crushed. On extraction being attempted, the blades of the cephalotribe slipped, and they were withdrawn and reapplied. One or two other attempts having also failed. Lyons and Sidey's forceps were used to diminish still further the size of the head. The cephalotribe was reapplied, and the crotchet having also been hooked on to the foramen magnum, traction on both was made simultaneously, and the woman was successfully delivered. The following day the patient complained of some pain about the region of the hip joint, but it did not seem to be very severe. She was pretty weak, but otherwise quite well. She made an excellent recovery, without a single bad symptom.

Appearance and History.—Patient is of small stature and delicate appearance, with a rickety conformation of the bones. Her pelvis is much flattened antero-posteriorly.

Her aunt states that she has always been a little feeble in intellect,—"soft," as she expresses it. Her mother was a healthy well-formed woman, and died some time ago of fever. Her father is described as "a drunken scoundrel."—Owing to his dissipated habits, the mother had to go out and work, and the children were in consequence much neglected. The patient was a very weak child, and unable to walk till she was seven years old. She has four brothers and sisters, of whom two are similarly deformed, an elder sister, and the youngest, a boy. The other two are quite well formed. Patient has had no previous confinement. Her mother's confinements were all perfectly natural.

Dr Keiller also alluded to another case, in which, when extracting a putrid child, the head was left in the uterus. It was extracted by the cephalotribe with great difficulty.

ON THE USE OF THE CONSTANT (VOLTAIC) CURRENT IN PAINS OF PELVIC ORIGIN.

By William Stephenson, M.D.

June 28, 1871.

One of the most important questions in practical medicine at present under discussion is that regarding the influence of the constant current of electricity over pain. On the one side, we have many observations which go to prove that in "the constant current we have a means more powerful than any other of modifying the nutritive conditions of parts that are deeply seated;" and, on the other, there are those who still refuse to grant to electricity any well established position in therapeutics, denying that

there is yet any satisfactory scientific basis for its employment, or any clear diagnosis of the cases where it is likely to be of service.

The profession in general are too apt to judge the question from the experience of former years, while yet the laws of electricity were but partially kown, while instruments were very imperfect in kind, and the agent applied in a haphazard manner. Within the last few years, however, a complete revolution has taken place,—new instruments have been invented, the laws of electrophysiology have been investigated, the principles for its proper application in disease have been established, above all, an essential difference has been demonstrated between the action of primary and induced currents,—so that the question stands upon an entirely different basis, and it now awaits the result of clinical observation to determine what is the position which electricity shall hold as a therapeutic agent.

Having used the constant current for two years in a large number of different affections, I would lay before you the result of my experience, in so far as it relates to certain classes of cases, which come more directly within the domain of this Society.

The first class to which I would refer is, the sacral and iliac pains which are met with so frequently in practice. Many of them are purely neuralgic, others muscular, while some are a combination of both forms. Their connection in a large number of cases with some abnormal condition of the pelvic organs is undoubted, but it is no less true that they often persist long after the primary local cause has disappeared. They are often extremely obstinate in character, and resist all our endeavours to remove them, and they are constantly suggesting the idea that there must be still some pelvic mischief keeping them up. Much malpraxis in this way obtains a shadow of excuse. In other cases some abnormal uterine condition may co-exist, but does not stand to the pains, in the relation of cause and effect, though often mistaken

for such. The decision of their true relation is a matter of great difficulty, yet nature at times solves it for us, by removing the suffering, while yet the local change remains as before; and a displaced uterus, and a scanty menstruation, or some pelvic adhesion ceases to be a source of discomfort to the patient. The difficulty which we have hitherto had to contend with is, that we did not possess the means of acting directly and locally upon the weakened state of the sacral nerves, which is the true cause of the suffering, or of determining the relations between the pain and the pelvic change.

Here, however, the constant current of electricity steps in. Its effect in alleviating pain affords immediate relief, and by its restorative influence, gradually removes the affection, while in it also we have the means of determining how far any local mischief is acting as an existing cause. In some cases where a cure cannot be looked for, great relief to suffering can be given more quickly, and with better effect, than by the use of anodynes.

The first case is one where retroversion of the uterus was accompanied by the usual train of symptoms. No attempt at rectification was made, and the result of the treatment shows that we may relieve the neuralgia without interference with the uterus.

CASE I.—Mrs U., æt 37, of a heavy, lax habit of body; married; has had six children, the youngest 14 months; not weaned. She has not menstruated since the birth of the child. For nearly three months she has suffered from pain in the back and left iliac region, with a bearing down feeling passing round the loins. She is easier when lying, but the pains are never entirely away, and are always aggravated by heavy work. During the last fortnight the suffering has so much increased that she is hardly able to perform her household duties; when walking in the street, every touch of the heel on the ground is felt on the back and side.

Examination.—Perinæum considerably torn, uterus low in the pelvis, cervix short, with os directed forwards.

Behind it a hard resisting body is felt. The sound passes to normal length, but backwards and to the left side. Uterus mobile; position easily rectified, but it at once falls back to its former place.

A direct current from twenty-five cells was passed through the region of pain for five minutes. The pain in the side was at once relieved, and she felt only a slight uneasy feeling in the back on stooping.

Next day the improvement continued; she had felt no pain, but a weary feeling in the back and limbs. The current again applied as before.

The patient did not again return, because, as she afterwards informed me, she was so much relieved she thought it unnecessary. The pains, however, returned at times, but never to such an extent as to inconvenience her, and she shortly afterwards became pregnant.

CASE II.—Mrs A., married for eight years, has never been pregnant. Seven years ago she had some inflammatory affection of the pelvis, and since that time has constantly suffered from pain in the back and left iliac region, subject to frequent exacerbations, and never entirely away. Twelve months ago I attended her for an attack of parametritis, which resolved without suppuration, and left no impairment of mobility of the uterus. The pains, however, were subsequently greatly aggravated; at times she could hardly walk, and even on sitting down felt as if something touched a tender internal spot.

August 13, 1870.—Menstruation somewhat irregular and scanty, pale and watery; cervix uteri small, the slightest pressure on uterus causes great pain, position normal, no thickening or tenderness in the neighbourhood. The tender condition she states has existed since the first inflammatory attack, and has always rendered intercourse painful. She complains of a rasping pain under left breast, a dull aching over sacrum, and a pain in the left iliac region.

A current from twenty cells removed the pains after each application; and after a month's occasional use, the relief

was permanent. She could perform all her house-work, and walk about with comfort. She also menstruated more abundantly, and with less of the ordinary pains. The tender condition of the uterus however remained, and was unaffected by a direct application of the current of the organ.

Five months afterwards there was a slight return of the pains, which were again removed by the electricity. Shortly afterwards she had a third slight inflammatory attack which soon passed off, after which the tenderness of the uterus spontaneously disappeared. The relief she ascribed to a mixture containing bromide and iodide of potass—but she had formerly taken these remedies without effect.

The pains in this case might be regarded as arising from the sensitive condition of the uterus, but I incline to look upon them as the result of the primary attack of parametritis.

Here is another case, in which there were decided abnormal pelvic relations, the pains from which were relieved under very unfavourable conditions, viz., poverty, an anxious mind, and hard work.

Case III.—Mrs D., æt. 40, married, two children.

Since birth of last child, four years ago, has felt a down-bearing in front, and at times something slightly protruding, especially after lifting a weight or walking about. Menstruation regular, no leucorrhœa. She also suffers much from pain in the back, always increased by fatigue; and at night when she turns in bed it is so severe as to make her at times cry out. For two months the suffering has been much increased, and has rendered her almost unfit for work. There is a feeling of numbness and dragging of the right leg.

Examination.—Slight prolapse of anterior wall of vagina; uterus large and rather low down, leaning backwards; os uteri somewhat patent, lips slightly swollen and soft.

The electricity was applied three times, when the patient stopped her attendance. Three months after-

wards I saw her and recorded this note. Has scarcely had any pain in the back since the use of the current—occasionally when unwell, or after heavy work, but soon left. Has been able to work comfortably, except for the leg, which is still numb and drags; the down-bearing has been much less. No pain in turning in bed since first application.

In the following case, the uterine displacement and habitual reflex irritation were such, as to render anything like permanent relief quite hopeless before the menopause. The effect of the electricity is on this account all the more valuable, as showing its wonderful power in affording relief.

Case IV.—Mrs B., æt. 42, has suffered greatly from ovarian and uterine irritation during her whole menstrual life. She has been subject to epileptiform fits at each catamenial period; but for three or four years past they have not been so frequent or regular. She has prolapsus uteri, and frequently suffers from an acute pain over the right ovary, extending down the thigh. Anodynes of various kinds, externally, internally, and by subcutaneous injection, have all been tried, but only afforded very partial relief.

August 7, 1869.—Complains to-day of severe pain in lumbar region, passing round the right hip and down the thigh to the knee. It makes her quite lame, and she cannot bear the weight of the body on that leg. It was with great difficulty she walked here a distance of half-a-mile. A direct current, increased gradually to twenty cells, was applied from the spine to iliac region and the thigh, after which she could walk freely. There was still a slight pain in the groin, now in the back or leg.

August 8.—Walked home yesterday with comfort; pains have not returned, but a knawing weakness in back and thighs. Twenty cells again applied. Uneasiness in thighs removed, back still feels weak.

August 9.—Feels altogether much better. Twenty cells as before.

August 12.—Improvement continues; to-day she has had a heavy washing without any return of pain, whereas on former occasions it always produced much pain and fatigue. Twenty cells direct.

August 17.—Continues stronger since last visit, no return of the pains. Twenty cells inverse.

Did space permit, several cases might be given to illustrate the relief obtained, in submamary pain of pelvic origin, in ovarian irritation persistent after menstruation, and also in cases where there was a chronic inflammatory condition around the uterus. The permanency of the relief in such cases is proportionate to the persistence and amount of the primary source of irritation. While the pains in such cases are liable to return, the severity is always lessened, and considerable periods of immunity can often be obtained.

There are many cases of backache which have their origin in the irritation of the menstrual period, but where a proneness to fatigue in the lumbar muscles renders the affection more or less permanent. Otherwise they are unassociated with any uterine aliment. An error in diagnosis, however, may be readily made, as it is easy by a leading question to get some acknowledgment of the vague term "down-bearing." Their real nature is muscular fatigue. The following two cases are good examples, and illustrate the beneficial influence of the constant current in restoring tone to the muscles.

CASE IX.—Mrs G., a widow, suffering from pain in the back, sickness, headache, and palpitation. The pain is a dull, weary aching in the lumbar region, frequently passes round to the left side and down the leg. During menstruation, and for some days afterwards, she can scarcely move about. At other times, she is well in the mornings, but fatigue always induces an attack of the pain. She is engaged as a missionary, and in the course of a day has to climb a great number of stairs. So much has she suffered, that she has seriously contemplated giving up her situa-

tion. At night she cannot lie on her back or right side, nor can she keep her leg extended while lying.

September 12th, 1870.—To-day the pain is so severe, she could with difficulty walk to my house, a distance of half-a-mile.

Direct current from ten, gradually increased to twenty-five, cells passed from lumbar region to pubis and down both thighs. The pains have been entirely removed.

September 14th.—Since application, has only felt at times a weary feeling in the back, but no pain. She has been walking about without difficulty. The first night she felt no pain while lying in bed, but last night could not lie on her back. Electricity repeated as before.

September 21st.—Menstruated last week with only an ordinary amount of pain. The back and side, however, have been very bad since; had great difficulty in walking yesterday. Again relieved by the electricity.

September 23d.—On her way home after last visit, had severe darting pain in the back for about a minute. To-day had much suffering while walking. Application repeated.

September 24th.—Had much walking to-day, pain returned in the afternoon. At once relieved by the electricity.

September 26th.—Since last visit, back has been free from pain. Yesterday morning had a severe pain in left ilium, passing down the front of thigh. Very severe to-day. Current applied to affected part.

September 29th.—No pain since last visit, and has had much work. Application renewed.

October 5th.—No return of pain. Since this time I have frequently seen her, and she has continued to perform her work with comfort. When greatly fatigued the back would ache, but the pain has never continued after rest.

CASE X.—Mrs B., æt. 44, wife of a mechanic, has had nine children. Menstruation regular. Nor morbid uterine symptoms. For eight years, has had a pain in shoulder; at times, for three or four weeks, it would be easy, but never

entirely away; at other times severe and darting through to the front of the chest. The chief seat is over the scapula, from the angle to the spine. It has greatly inconvenienced her in working, and she cannot pour out tea without supporting the arm. The pain has been very severe for three weeks. Six years ago, she had an attack of sciatica, which yielded to the use of Corrigan's button. Since then, she has always felt a pain in the lumbar region whenever she attempted to do any work. Often, in doing her household duties, she has to sit down on account of it, and has never been able to wash.

7th November 1870.—Fifteen cells applied to shoulder and arm.

9th.—Has been entirely free from the pain, not even the feeling of weakness in the arm. Has been using it freely. Fifteen cells again applied to the shoulder, and also to the back.

December 5th.—Arm continues free from the pain. The back also has been greatly improved. Application renewed to the back.

12th.—Back has been very well since last visit, and she has been able to perform her work with comfort. For the last two days, there has been a pain in the hip and round the crest of the ilium. Ten, increased to fifteen, cells applied.

June 1871.—Since last date, she has been quite free from pain, has been able to do all her house-work, and has dispensed entirely with her washerwoman, performing herself the washing of a large family.

The following case is a marked example of the permanency of the enfeebled nervous condition, arising from pelvic mischief, long after the exciting cause has passed away. These "*after pains*" of uterine disease, as Dr Till aptly terms them, are very common, and exceedingly obstinate under the ordinary means of treatment. Nothing could exceed the constant current in its direct and beneficial effect in such cases, when once all active disease has disappeared.

CASE XI.—Elizabeth M'D., æt. 22, unmarried. Had a

child eighteen months ago, after which she was confined to bed for two months by pelvic inflammation, the pain of which was principally seated in the left iliac region. Since that time she has never been well. She has often tried to work, but had to give up her situations on account of vomiting and sickness, and pain in the side and left lower cartilages. Rest always improved her condition, but work was sure to bring a return of the sickness. Although at times improved, she has never been entirely free from pain, especially in the back and iliac region. In rising from a chair she has always to help herself with her arms, and cannot turn in bed without pain. For the last three weeks she has been in a situation, but has vomited every day, and has been sent by her mistress to the Royal Infirmary. She was admitted (June 24, 1870) under the care of Dr Matthews Duncan, who finding no pelvic disease, sent her to me to be treated by electricity.

June 27.—Since admission she has been constantly sick, and vomited frequently every day. The pain in the back has been easier from the rest, but there is pain and tenderness in the left iliac region, and a general tenderness over the abdomen. There is great sensitiveness over the coccyx, which she says has prevented her sitting any length of time at once. A direct continuous current from twenty-five cells was applied to the length of spine and through the pelvis for ten minutes.

June 29.—Patient states herself as much better. She has been free from nausea, and only vomited once, and that after assisting the nurse in the ward. The sickness and vomiting, she says, never left so suddenly before. She slept soundly all night, the first time for three weeks. She has also been able to turn herself in bed totally free from pain. The pain in the ribs rather severe to-day. Fifteen cells direct continuous current to the spine, afterwards inverse to spine and sympathetic. The position pole was applied to the coccyx for some minutes, after which she could bear hard pressure upon it.

June 30.—Has been working about in the ward without

any return of the sickness. She has also sat for two hours at a time, which she could never do before, and can rise from a chair without assistance. The pain in the groin was present for an hour this morning, but has gone; there is now no tenderness on pressure. Electricity applied as before. It was again repeated next day, when she expressed herself so well that she was anxious to return home.

To obtain these results, what is wanted, is not a current of great intensity, but one with a large quantity of electricity of low tension, and twenty-five Daniels' cells are sufficient for ordinary cases. The mode of application I adopt is by two large surface sponge conductors, the one placed upon the spine, the other on the iliac regions, and the inner side of the thighs. I have tried the direct applications to the uterus by a vaginal conductor, but I did not find any well marked advantage over the external method, and it is much less agreeable to the patient. Each application should last five or ten minutes, and the direction of the current should frequently be changed during the sitting.

Drs Keiller and *Gordon* took part in the discussion.

Dr Stephenson, in reply, said that a great point in applying the current was to send it through the affected nerve. What is wanted for success is a current of low tension and large quantity; and said he would be happy to treat any cases under supervision of medical attendant, at his own house, without fee, and to give any of the Fellows any information he could regarding this method of treatment.

GALL BLADDER AND SEVERAL GALL STONES.

By A. M. T. Rattray, M.D., Portobello.

July 12, 1871.

Dr Rattray showed a gall bladder and several gall stones, which were carefully examined and dried by Dr Littlejohn, who performed a post-mortem examination on the body of the female from which they were removed.

OBSERVATIONS ON THE INFLAMMATIONS OF THE MOUTH IN CHILDREN, WITH AN ILLUSTRATIVE CASE OF CANCRUM ORIS.

By R. Peel Ritchie, M.D.

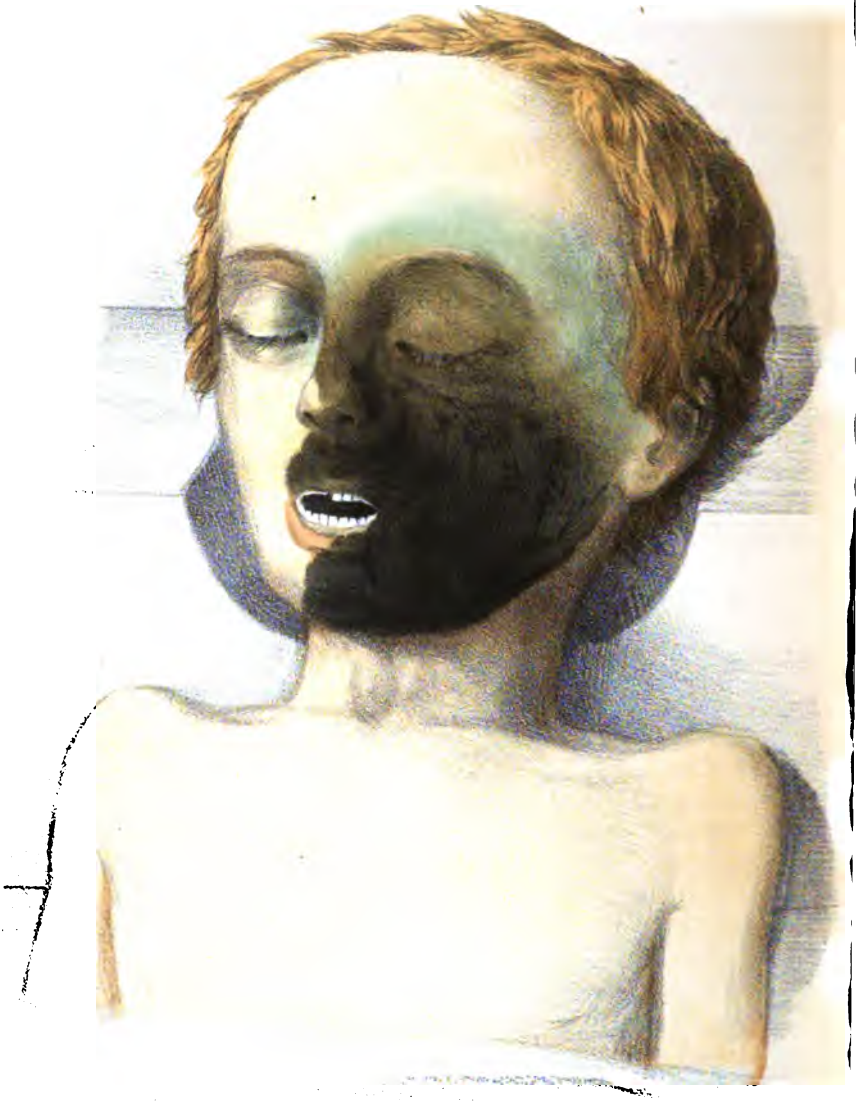
July 12, 1871.

Before submitting the report of this case of Cancrum oris to the Society, I desire, as shortly as possible, to direct the attention of the Fellows to some general questions regarding the inflammations of the mouth in children. The *first* of these is—

1. *The Names of these Diseases.*—It is to be regretted that at present the names of the various forms of stomatitis are employed by authors to indicate different diseases. Thus the name "noma," from νομη, that which devours, *e.g.*, an ulcer, is used by some to embrace generally phagedenic ulceration of the mouth and face as well as of the vulva; whilst, by other writers, it is restricted to the affection of the latter, although the ulceration destroying that part may be considered to be dependent upon a similar or even identical constitutional state with that producing the buccal ulceration. By a third class of authors, noma is probably limited to the ulcerous or ulcerative stomatitis; and by a fourth, it may be restricted to the gangrenous stomatitis. The reports of the Edinburgh Children's Hospital, founded on the form of the Registrar-General's Returns, also show our confused ideas regarding the employment of his title.

Again, the term "cancrum oris" is, by some, held only to embrace the ulcerous form of stomatitis; whilst, by other authors, the ulcerous and gangrenous forms may be held to be merely different stages of the same disease. And again, yet others restrict the name to the gangrenous variety; and by a fourth, the disease is held to be distinct from any of these forms of stomatitis.

The term "aphthæ" is also very indefinitely used, and



From Postmortem Sketch by Dr W. Livesay.

Mc Farlane & Erskine, Lith.rs Edr.

includes diseases which by some are considered to be distinct.

It is time, therefore, that clearer views were held as to these various diseases. On turning to "The Nomenclature of Diseases, drawn up by a joint committee appointed by the Royal College of Physicians of London," 1869, the arrangement of diseases of the mouth does not appear to me to be quite satisfactory. I would suggest, therefore, the following classification. If the generic term "stomatitis" were adopted, and "noma," as specially applied to diseases of the mouth, discarded, most authors, if not all, are, I believe, agreed that the following species are recognised:—

Generic Term, *Stomatitis*, Inflammation of the Mouth.

1.	Stomatitis, Simplex, .	Simple Stomatitis.
2.	„ Vesiculosa, .	Vesicular Stomatitis or Aphthæ.
3.	„ Parasitica, .	Parasitic Thrush.
4.	„ Exulcerans, .	Ulcerative Stomatitis.
5.	„ Gangrænosa, .	Gangrenous Stomatitis or Cancrum Oris.
6.	„ Mercurialis, .	Mercurial Stomatitis.
7.	„ Diphtheritica,	Diphtheritic Stomatitis.
8.	„ Scorbutica, .	Scorbutic Stomatitis.
9.	„ Syphilitica, .	Syphilitic Stomatitis.

The contagious "aphtha," or "eczema" of cattle, also affects human beings. To render the list more complete, the form "epizootica" might be added.

If these terms were generally employed, and they seem to be correct enough, there would be less difficulty in understanding the writings of authors; as long as some of the terms are used so indefinitely, it is not difficult to understand why our progress in the pathology of the diseases of the mouth is so little advanced. I also further suggest that the term "noma" should be restricted to indicate that peculiar *constitutional state* in which gangrene of a part in children appears to arise spontaneously. I have seen it affecting the tissues of the mammary region after measles; and it also attacks the mouth, vulva, and other parts. Noma,—the idea being taken from the clearing of a pasture by a flock,—may therefore be taken to repre-

sent progressive spontaneous gangrene, as that disease extends from a spot first affected. In this view, we would speak of "noma oris," or "noma pudendi," or "noma mammæ," or according to the part affected. In this sense, "noma oris" would be synonymous with "gangrenous stomatitis" or cancrum oris.

The ulcerative or ulcerous stomatitis, as it generally occurs in children, might, with propriety, however, be subdivided into two varieties. In children, I am convinced that ulcerous stomatitis commencing in the gums is frequently of scorbutic origin. My attention was very strongly called to this by the frequency with which children so affected were brought to the Children's Hospital Dispensary during and towards the end of the prevalence of the cattle plague. These children were supplied with their usual amount of milk, but they could not have been with the necessary quality. A peculiarity too was, that cases of this nature occurred at an earlier age than ulcerous stomatitis is usually met with. Some of these might possibly have been of epizootic origin, for, in the increase of the white blood cells, they resembled the observations made on the blood of diseased cattle. But others were not of that nature, and since the observation then made, I have frequently met with cases of this species which had a scorbutic origin. I would, therefore, subdivide this form into the scorbutic and non-scorbutic. I am of opinion, too, that this subdivision is not unimportant, for undoubtedly there is testimony to a stomatitis of mercurial origin becoming gangrenous. Thus Mr Cooper Forster, at page 24 of his work, "The Surgical Diseases of Children," observes, in his article on Cancrum Oris: "I am, however, disposed to believe that though these may be set down as predisposing causes, the indiscriminate use of mercurial powders, a portion of which very frequently is retained in the mouths of young children, is, in the larger proportion of cases, the exciting cause." Although I cannot adopt this explanation of the occurrence of cancrum oris, still it has seemed to me

probable that, if mercury were given to a child suffering from scorbutic stomatitis, a very serious consequence might ensue. I have not ventured, however, to put my idea on this subject to the test of experiment.

2. *The Nature of Cancrum Oris.*—In many of the species into which I have divided stomatitis, there is not much difficulty in understanding their origin and nature. In the gangrenous form, the more special subject of this communication, the pathology is not so clear. A considerable number of the authors I have consulted give the symptoms and general views of its occurrence. By some it is questioned whether it may not be merely an advanced form of the ulcerous or ulcerative species. By others it is considered as a local disease. But, by a third class, with whom I agree, the local is but regarded as a manifestation of a general disease. For the most part, all agree that the disease usually occurs in children of a certain age, placed in unfavourable hygienic conditions, and most frequently in those who have suffered from a continued fever, either of the eruptive or non-eruptive character, but perhaps oftenest met with in connection with measles. As may be presumed, these necessary malhygienic conditions are most frequently met with amongst the children of the poor. It is consequently amongst them that gangrenous stomatitis most largely occurs, and thus its occurrence in hospitals and institutions for children is accounted for.

There is a want, however, of precisely recorded pathological details; so far, at least, as I have been able to ascertain. I consider it is probable it is a gangrene ensuing from an unhealthy crasis of the mass of the blood. This view would appear to have support from these considerations. First, that the disease generally arises in those who have been debilitated, or had their vitality lowered by being subjected to some form of fever blood-poisoning; and secondly, that an analogous disease affects other parts, more especially the labium pudendi of young female children. Some remarkable cases are

detailed of apparently spontaneous gangrene in children where the disease was not limited to the neighbourhood first affected. It is questionable, however, if all of these were identical in nature with that under consideration. Its supposed connection with mercury I have already alluded to, but I would add a few remarks on its relation to scurvy. In the latter, the mouth affection begins in the gum, and the affection is not limited; whilst, in cancrum oris, the sloughing begins in the buccal wall, and is generally limited to the side it begins on. The teeth in scurvy, although loosened, have a tendency to be covered by the fungus gum; whilst in the other, they are exposed and fall out; they differ, too, in the increased pain, their constitutional symptoms, and the appearance of the blood.

Dr Keiller, in his valuable contribution to our knowledge of this disease (*Edinburgh Medical Journal*, vol. vii., new series, p. 929), observes,—"The occasional arrest of cancrum oris, either by topical applications, or the mere removal of the morbid mass, would, along with the facts now stated, tend to indicate a merely local malady, but, at the same time, although all this be true, and although the real nature of the origin and pathological course of this disease be involved in much obscurity, there can be little doubt as to its being very generally connected with a constitutional origin, a general or blood disease causing local death. Like local or symptomatic gangrene attacking the extremities, the parts involved are probably destroyed from innutrition, the course of the blood becoming suspended, the vessels being more or less impervious; the structures thus dying from want of their nutrient fluid."

Another view regards this disease as arising from local and general debility. Thus, Dr Robert Carswell, who adopts this explanation, observes (Cyclopædia of Prac. Med., vol. iii., p. 138),—"The most marked examples of mortification from general debility are met with in individuals whose strength is greatly reduced by want and fatigue, by the violence of acute diseases of an adynamic character,

and by chronic diseases, accompanied with much pain, extensive suppuration, or which compel the patient to remain for a long time in the same position." Under mortifications of this class, he includes that "of the soft parts covering the sacrum, heels, elbows, etc., of persons who have escaped the dangers of typhoid fevers, and who are left in that state of prostration which precludes the possibility of changing the position of the body." He also instances under this head the occurrence of mortification in scorbutus and cancrum oris. An objection to this view which occurs to me is, that if general debility were alone the cause, why should the mortification occur on one side of the face, and not have previously in any degree shown itself on those parts on which the child had been lying? In the case I have to read to you, the child had been continuously in bed for several weeks. After being up for eight days, he was sent home, and up to that time had not suffered from sores on any of the usual places. Being then convalescent from his attack of typhus, he was removed from the hospital in an apparently favourable state, after a residence of above fifty days. The change to his miserable home seemed to have at once affected him, and four days after the local disease attracted notice. Would we not, in Dr Carswell's view of the case, have rather expected that ulceration on the sacrum, heels, or elbows, would have previously shown itself?

That the gangrene directly arises from obstruction of arterial supply has also been suggested, but dissection has failed to establish this view. In one of the cases narrated by Mr Forster (*Op. Cit.*, p. 26), he observes,—"The vessels going to and coming from the diseased part were carefully examined, but nothing was decernible. The facial artery, where it entered the sloughing mass, was closed by a natural clot." The experience of other observers has been somewhat similar.

The difficulty in the disease which still presents itself, is to account for the gangrene occurring in the cheek, and being generally limited. If it is allowed that one of the

simpler forms of stomatitis precedes this graver one, then possibly the explanation of its local occurrence would not be so obscure, but as I am inclined to regard the disease as not being so preceded, I have no theory to offer. Could the pressure and irritation of some unmasticated portions of food, on the cheek on which the child lay, excite so grave a result in one whose state of blood was faulty? Might not a vitiated salivary secretion act as a local irritant? or a vaginal, in the case of the gangrene affecting the vulva? In the child I referred to, who had gangrene of the mamma, the only exciting cause the mother could imagine was the application of a poultice. A slight occurrence may therefore act as a developing cause.

Leaving speculation, however, as to the local exciting cause, a review of the circumstances in which the disease occurs would give some such result as this. Before the entire structures become gangrenous, the following phenomena may be observed. In a blood-poisoned child, the cheek becomes the seat of a circumscribed affection, which on the outside is extremely hard, and, at first, painful, with internally a corresponding portion of mucous membrane inflamed, and which subsequently becomes gangrenous; the induration of the cheek afterwards extends, and as it does so, the sensibility of the part, an important sign, is not only destroyed, but all the tissues become at last gangrenous; the originally affected circumscribed hard portion first dies, and from this, as a centre, the gangrene spreads. It does not, however, usually pass the mesial line, for as the nutrient supply to the other side of face is unaffected, the gangrene does not extend there, although I have seen it pass over. I presume the main arterial supply to the cheek is not interrupted in the first instance, although it subsequently becomes so from the pressure of the indurated tumour. The view I incline to is, that the pressure of this tumour causes the gangrene of the cheek, by at last stopping entirely the nutrient supply. Do not we find, in certain cases of injury, that (the exciting

cause being for the present set aside) a somewhat similar explanation of the occurrence of gangrene may be given? The main artery may be uninjured, but from the infiltration of the tissues from small branches being ruptured, the pressure upon the artery prevents the flow through it, and from the effects of extravasation in the first instance, and the stoppage of capillary and arterial supply in the second, the affected limb sphacelates. So, in the case of gangrenous stomatitis, the mucous membrane is affected, and there is effusion into the tissues of the cheek. The swelling is hard at first and circumscribed, but as it goes on increasing, it presses on, and at last cuts off the blood supply, and so causes the death of the tissues, whose nutrition has been suspended. Were the blood in a healthy state this result would not occur, but the blood being deficient both in quantity and quality, the peculiar kind of effusion in the first instance is permitted to occur. The result of local treatment throws also some light on this view. The most successful is that which rapidly destroys the primarily affected part—the explanation of this, I presume, being, that by the strong escharotics used, the extension of the fatal effusion is prevented, and the nutrition of the parts in the neighbourhood of the original lesion is again permitted. It will be seen, therefore, that I regard the peculiar induration* as being due to the morbid state of blood, and that the pressure of this tumour, along with that state of blood, interrupts, and at last destroys the nutrition of the part affected. My difficulty still remains to account for these phenomena occurring in the cheek, as I am not prepared to admit they commence with an ulcerative stomatitis of the ordinary character.

3. The *third* head to which I propose directing your attention is,—The *Frequency with which Cancrum Oris occurs in Edinburgh.*

The only reliable source from which information on this can be obtained is the Children's Hospital. I confine my observations, therefore, to the returns from the Children's Hospital, and do not take into consideration the

* As in some cases of Hæmophilia.

cases I have seen elsewhere. Since that institution was opened in 1860, the following cases have been admitted :—

1861.	1. Mary Ferguson, 150 Cowgate.	Aged 3.	Sept. 6.	Died Sept. 19.	Gang. Stomatitis.
,,	2. Chris. M'Millan, Coul's Close, 317 Canongate.	,, 8.	Oct. 22.	Recov. Nov. 16.	,, ,,
1866.	3. James Smith, 305 Cowgate.	,, 3.	Jan. 5.	Died Jan. 7.	Cancrum Oris.
,,	4. Elizabeth Reid, 138 Grassmarket.	,, 4.	Jan. 26.	Died Feb. 1.	,, ,,
,,	5. Bridget Hay, Bell's Wynd, 146 High Street.	,, 4.	Dec. 20.	Died Jan. 1.	,, ,,
1867.	6. Emma Hunter, 20 Kirkgate, Leith.	,, 8½.	June 26.	Recov. Aug. 27.	,, ,,
1869.	7. Betsy Sinclair, 13 N. Gray's Close, 125 High Street.	,, 3.	Sept. 16.	Recov. Nov. 23.	,, ,,
1871.	8. Wm. Paterson, Campbell's Close, Canongate.	,, 4.	April 21.	Died April 24.	,, ,,

Giving a total of 8 cases, with an average age of 4 years. Of these, 5 died, and 3 recovered.

The total admissions to 27th June 1871, equal 4521, this would give 1 case of gangrenous stomatitis or cancrum oris in 565 admissions. Of the 8 cases, 6 were females and 2 males. Of the 8 cases—

3 females and 1 male were between the ages of 3 and 4.
2 ,, 1 ,, ,, ,, 4 and 5.
1 female was ,, age 8 and 9.

The respective admissions at these ages were, between

	3 and 4	4 and 5	8 and 9
Males, .	231	251	187
Females, .	225	212	188

This gives—

1 male in 231 admissions, at 3
1 female in 75 ,, 3
1 male in 251 ,, 4
1 female in 106 ,, 4
0 males in 187 ,, 8
1 female in 188 ,, 8

These results appear to agree with the observations of writers on this disease—its greater frequency amongst females, and also its greater frequency under the age of 5 or 6, and between the ages of 2 and 6 years.

As being analogous in nature to cancrum oris, I may here observe, that two cases of gangrene of the vulva—both of which were fatal—were admitted during the years 1866 and 1867, the one was aged 19 months, and died in 4 days, the other was aged 2 years, and also died in 4 days.

The statistics of the out-door department of the Hospital are, perhaps, not quite of such value as regards cancrum oris, for cases so serious are generally sent direct to the Resident physician for admission, and do not appear in the out-door Register. Two cases of the disease are registered out of 45,578 cases—at all ages—between birth and fourteen or fifteen years. These 2 cases were not admitted to the Hospital. It is difficult to estimate how many of the cases received into the Hospital were registered in the Dispensary books; but, for the sake of argument, allowing that all were (they are 4521), and adding the cases treated in the Hospital and those at the Dispensary together, and dividing the cases by the resulting number, an idea of the rarity of the disease in Edinburgh may approximatively be obtained—$8 + 2 = 10$. $\frac{50092}{10} = 5009{\cdot}9$, so that the disease may be supposed to occur about once in 5010 cases of children's illness in this city.

In the Hospital reports, the term noma is vaguely used. An attempt has been made to separate stomatitis and noma, but they are so mixed together, no reliable information can be gained from them separately. In 1867, one case of stomatitis, a male aged two, died in three days in the Hospital—the species of the stomatitis does not appear. During the first four years of the Hospital, "stomatitis" does not appear in the report of resident cases—all the cases were included under noma. During these years 27 cases of noma were admitted. From 1864 to end of 1870, two cases of gangrene of vulva were admitted,

and 30 cases of stomatitis, or a total of 59 out of 3942 admissions, or 1 in about 67, at all ages.

In the Dispensary Report, in which noma and stomatitis have always been conjoined, out of 45,578 cases, there were 513 classed under this head, or 1 case of stomatitis in about each 89 registered. This gives a very different result from the cases of cancrum oris, and furnishes an additional argument for regarding that disease as a distinct form of stomatitis. For, if it were but an advanced condition of ulcerous stomatitis or noma, it surely ought to be more frequently met with.

Turning now to the Hospital for Sick Children in London, we learn from Mr Holmes' excellent work on the Surgical Treatment of Children's Diseases, that "out of 4999 patients admitted into the hospital as in-patients, only 7 are entered as cases of "cancrum oris." This gives 1 case of cancrum oris in 714 admissions. Of these 5 recovered and 2 died."

4. *The Effects of Season on Stomatitis.*—Season has, by some, been presumed to have an influence on the diseases under consideration. Of the 8 cases of cancrum oris treated in the Hospital—

2	occurred	in January, . .	2 died.
1	„	in April, . . .	1 died.
1	„	in June, . . .	1 recovered.
2	„	in September, .	1 died—1 recovered.
1	„	in October, . .	1 recovered.
1	„	in December, . .	1 died.

This little information can be gained from this return, that 4 of the 5 fatal cases died in the winter and spring months.

As regards the frequency with which stomatitis and noma occurs, out of 393 cases in the Dispensary of the Hospital during seven years, when correct tables were prepared—

43	occurred	in . . .	January.
34	„	in . . .	February.
26	„	in . . .	March.
31	„	in . . .	April.
30	„	in . . .	May.

29	occurred	in . . .	June.
33	„	in . . .	July.
34	„	in . . .	August.
45	„	in . . .	September.
27	„	in . . .	October.
35	„	in . . .	November.
26	„	in . . .	December.
393			

This table gives the information, that season in Edinburgh appears to exercise but little effect on these diseases.* And as regards years; in

1863,	48	cases were treated.	
1864,	47	„ „	
1865,	62	„ „	(Cattle plague year.)
1866,	53	„ „	
1867,	81	„ „	
1868,	50	„ „	
1869,	52	„ „	

5. The *last* head I have to bring before your notice is that of *Treatment.* In the case I shall read to you, treatment was of small importance, the child being moribund when admitted. Wishing, however, to try the effect of the citric acid on a disease which I regarded of blood origin, he was at once placed on iron with citric acid.

* In connection with stomatitis, I may here state, Principal Williams informs me no Government returns of the Foot and Mouth Disease in Cattle were required until after October 1869. He writes me:—"The disease is always most prevalent in the spring and autumn, which is due to the facts that animals are removed at these times to and from their pastures, and that the cattle trade is then most brisk.—In some instances the mammary glands of cows are affected with the disease, that vesicles form both within and without the teat, and that the contents of such vesicles are pressed during the operation of milking into the milk. This, I think, is of itself sufficient to account for such milk affecting human beings, dogs, etc." For the effects of the disease on man, I may refer to Dr G. W. Balfour's very interesting remarks on Mr J. B. Hislop's "Two Cases of Murrain (Aphtha Epizootica) in Man," *Edinburgh Medical Journal*, July 1863; and to Professor Brown's (*Veterinarian*, December 1869) "Foot and Mouth Disease in relation to the Meat and Milk Supply."

The result was negative. More importance was placed on support and stimulants. Nutritious enema, too, were tried, but had to be soon discontinued. The application of strong acids, or the cautery, as advised by those who have had opportunity to treat cancrum oris at an early period, were, I considered, unadvisable in the circumstances.

Amongst the articles of the Materia Medica—recommended internally—are iron, ammonia, quinine, sulphur, chlorate of potash. Unless, however, the case is seen at an early period, I fear little reliance can be placed on any one of these. The early application of escharotics (or, as recommended by Coates, of strong solution of sulphate of copper,) appears to offer the best chance for the patient, as I have already stated; but local treatment alone can not be depended on, as recorded experience shows that a fatal result may occur even with local improvement. Looking to the post-mortem appearances presented by the subject of the following case, I should say the quantity as well as the quality of the blood must be considered, and the means calculated to increase the former are quite as essential as those to improve the latter.

For the following report of the case which has led to the foregoing observations, I am indebted to Dr Alexander Matthew, the resident physician at the Sick Children's Hospital:—

William P., aged 4, was originally admitted on January 24, 1871, to the Royal Hospital for Sick Children, suffering from Relapsing fever. He was at first under the care of Dr Linton, but in the interval between the end of the primary fever and the beginning of the relapse (February 1st), he was transferred to the care of Dr Ritchie. The child made a good recovery, and was about to be sent home, when he showed symptoms of Typhus, and was put back to bed. From this date (March 4), he went through a regular but mild attack of typhus, and had so far recovered as to be allowed up about the 21st day of

the month. He was dismissed from the Hospital on March 29th, and was taken to a home of a wretched description in Campbell's Close, Canongate. The house consisted of one room, ventilated only by the doorway, and with its windows looking out upon a dark wall about 12 inches distant. In this dark recess, with bad air and little food, the child soon became ill again. On the 2d April, a swelling was first noticed in the left cheek, and for this he was treated by Dr Dixon, who observed a small livid spot of ulceration on the mucous membrane of the cheek, and regarded the case as one of stomatitis. The swelling externally was hard to the touch. Soon a gangrenous spot appeared, and Dr Dixon strongly and frequently urged on the parents the necessity of sending the child to the Hospital, but they persistently refused, even although they saw the gangrene extending. They yielded so far as to allow the child to become an out-patient at the Infirmary, and the slough was treated chiefly by charcoal poultices. The gangrene, however, increased in extent, and after the lapse of nearly three weeks from the commencement of the disease, his mother having in meantime succumbed to an attack of fever, his father was only too glad to have the child admitted to the wards of the Hospital.

He was admitted on the evening of April 21.

Condition on Admission.—On approaching the child, one was met by the foul fœtor emitted by the gangrenous matter, and by the stale poultice which covered it. The child's breath, too, was very fœtid. On removing the poultice, it was at once seen that a large portion of the left cheek was a black flattened slough. The parts involved were the following:—The whole of the left cheek in front of a line drawn from the outer canthus of the orbit downward to a point midway between the angle and the symphysis of the lower jaw, the left half of the upper and left half of lower lip, the left ala nasi, and the lower eyelid. At the margins of the slough, the skin was œdematous, and presented a dirty green colour.

The right side of the face was pale and waxen. The tongue was coated with a white fur—the mouth could not be thoroughly examined, on account of the weak and peevish state of the child, but the left canine and first molar were loosened. He was perfectly able to swallow fluid that was put in his mouth.

The chest was natural—respiration somewhat slow, and breath very fœtid.

Pulse 148, very feeble. Temperature 101°.

Was ordered to have enemata of brandy and beef-tea every two hours, and brandy and milk to drink, and a mixture of iron with citric acid. Lint soaked in a strong solution of citric acid was ordered also to be constantly applied to the gangrenous parts.

April 22.—The enemata ordered last evening were discontinued after two trials, on account of their not being retained. The child has partaken freely of the brandy and milk passed into his mouth by means of a feeding cup. Slept well during the night, but awoke occasionally with a cry.

Pulse not noted—still very feeble. Temperature 98°.6. No change in the state of the face. Ordered egg-flip.

Vespere.—T. 100°.8.

April 23.—P. 148, T. 98°.2. The gangrene has now extended—the ridge of the nose has been crossed, and the right eyelids are œdematous. The posterior margin is also slightly extended.

Vespere.—T. 98°. Child continues to take the egg-flip, and brandy and milk in addition. Seems sinking.

A small amount of urine (about ʒi.) was obtained to-day, it was of a black colour, apparently due to the presence of blood, and was highly albuminous.

April 24.—T. 93°. The child's skin is now very cold—the pulse imperceptible. The gangrene now implicates the left upper eyelid, and even the eyebrow; it also extends backwards towards the external ear. The whole of the nose is involved, except a small portion on the right side of the tip. The shading between slough and

healthy skin is still of the pale green shining appearance.

Vespere.—At 5 P.M., the temperature of the axilla was below 90°, and the pulse was imperceptible. The child died at 7 P.M.

Sectio Cadaveris, April 27.—66 hours post mortem.

External Appearances.—Body much emaciated. Whole of the soft parts on the left side of face in a state of black slough, tolerably firm towards the edges, but becoming diffluent in the cheek, outline of masseter defined. The boundary of the slough was as follows:—Beginning at a point in the upper lip, a quarter of an inch from the right angle of the mouth, it extended upwards to the nose, included the septum nasi, passed upwards and to the right side (leaving the right ala, and right half of tip unaffected), extended to the inner canthus of the right eye, then crossed the bridge of the nose to the left eyebrow, then backwards and downwards to within half-an-inch of the tragus of the left ear. From this point it extended downwards and forwards to the lower surface of the lower jaw, then forwards in the middle line, and upwards across the symphysis to the middle of the lower lip. The edges of the slough were in a state of green putrescence. The teeth in the upper and lower jaws of the left side were all loose, and one or two had dropped out. On probing, it was found that nearly the whole of the left superior maxillary bone was in a state of necrosis, as was also the inferior maxillary bone from the symphysis to the angle—that is, these bones were exposed.

The skin generally was anæmic. On the forearms and abdomen several petechial spots were observed.

There was found to exist marked anæmia of all the internal organs, with slight congestion of the ileum.

On examination of the kidneys, Professor Sanders found "that the epithelium cells of the cortical tubules were in the stage of cloudy swelling, *i.e.*, swollen and granular."

The bladder contained about ʒii. of smoky (almost

black) urine, which, under the microscope, was found to present blood corpuscles and granular tube casts. The urine of course coagulated on boiling.

In the brain there was sub-arachnoid effusion. The left olfactory bulb and left frontal lobe were perhaps softened—but this is doubtful.

In performing the examination, there was observed to be a marked deficiency of blood, and the small amount which was observed was remarkably fluid, of a pale colour.

Dr Keiller said that he entirely agreed with the observations made by Dr Ritchie, as to the necessity of altering the classification of the various branches of this subject. There is a vast difference, for instance, between the simpler and graver forms of stomatitis, and they require very different modes of treatment. He had seen at least five cases of well-marked gangrenous stomatitis, the cast of one of them being at present on the table. One chief object of treatment was to get rid of the putrid gangrenous mass if possible, for he was of opinion that the constant inhalation of foul air was most injurious to the patient. If, by application of nitric acid under chloroform, or of carbolic acid, we can succeed in destroying the gangrene early, the child might be saved from being poisoned. He was of opinion that the disease most commonly had its origin in fever, or some depressing influence. He also alluded to other cases which had recovered.

Dr Cochrane said, that in this mysterious disease, which produced such dreadful results, he thought stimulants should be given freely.

Dr James Young said, he had never met with any case like Dr Ritchie's, but had seen numerous cases of ulcerative stomatitis, in which extensive destruction of teeth and bone resulted. In all such cases, he was in the habit of using carbolic acid, applied by means of a glass rod, and its beneficial effects were most marked. He also gave stimulants and nourishing diet.

Dr Stevenson Smith said, that two fatal cases of cancrum oris had come under his care when Resident Surgeon at the Hospital for Sick Children. He exhibited photographs

of the cases taken from casts. In both cases there was extensive destruction of the face, etc., and although carbolic acid and many other remedies had been used, the gangrene progressed, and both patients died. In one of these cases, he thought it had been pretty clearly made out that the cancrum oris had been preceded by an attack of simple ulcerative stomatitis.

Dr Thomson said, that several years ago he was called in consultation to see a boy five years old, who was then apparently hopelessly ill from gangrenous stomatitis. The inside of the mouth was covered with a dirty ash-coloured deposit, and no food had been swallowed for several days. He recommended the application of the solution of chlorinated soda, which had a marvellous effect in cleansing the mouth, and in a short time the child could swallow, and ultimately recovered.

CASE OF PUERPERAL SCARLET FEVER.

By James Young, M.D.

July 12, 1871.

Mrs G., æt. 25, was married on the 13th April 1870. She informs me that she has always enjoyed good health, and that her menstruation was regular, until the last week of July 1870, when it ceased. A male child was born on the 16th April 1871. The labour, which lasted from 3 P.M. until 9.30 P.M., was natural and easy, with normal presentation. The child was well developed and healthy. On the 17th, the mother seemed quite well, and gave promise of a good recovery. On the morning of the 18th, Mrs G. was seized with a severe rigor, and on my visit I found her very feverish, and complaining of intense nausea and giddiness, with severe uterine pain, followed by complete suppression of the lochial discharge, pulse 100, and tongue furred. These symptoms continued until the next day (the 19th), when a scarlet eruption spread over her face and neck, rapidly extending over the body. Tongue somewhat red, the throat slightly inflamed

urine very scanty, intense thirst, total disinclination for food, pulse 130. The uterine pain continued for two days, but was relieved by the application of turpentine stupes. The treatment pursued at this stage was simple, and consisted of the occasional administration of mercurial purgatives, and moderately warm covering, with drachm doses of the Aqua acetatis ammoniæ every three hours. The scarlet eruption was well developed for five days. On the 20th, the pulse rose to 136, and the patient became somewhat incoherent. No application to the throat was necessary. The treatment was now considerably changed, and the usual dose of the Tincture of the muriate of iron was commenced, and repeated every three hours. A large glass of claret was given four times a day; beef-tea and milk nearly *ad libitum;* chlorate of potash in scruple doses, with hydrochloric acid in water, was given frequently as a drink. Brandy was substituted for the claret on the 26th, and half a glass was given in water every three hours till the 28th, when the pulse began to fall to 124. The patient made a slow but perfect recovery, being confined to bed until the 15th May, by which time she was able to sit up a little every day. Desquamation of the cuticle commenced on the 6th May. There was no dropsical effusion, and never any secretion of milk. She did not menstruate for three months after her recovery.

Dr Ziegler remembered being called one day to see a lady, a patient of his father's, about to be confined. He went directly to her from a scarlet fever case. Two days afterwards the scarlet fever rash made its appearance, and the disease followed a simple course. The baby likewise took the fever, and both recovered. He remembered another case, where scarlet fever broke out in the family of a woman who was just about to be confined. He asked the patient to have the child removed, and to get some one else to attend her, but she would do neither. Shortly after delivery she was seized with rigors, phlegmasia dolens set in, and she died.

Dr Keiller remarked, that if lying-in women got over the first week well, they are not so liable to take infection

after that time. He also remembered a case where he was under the impression that he had been the means of carrying the infection. He had been attending some scarlet fever cases, and went to deliver a lady. Three days after she showed symptoms of the fever, and nearly died from it. A sister also took it, but recovered.

Dr Thomson remembered a case of puerperal scarlet fever recover which was supposed to be hopeless. In that case the late Sir James Simpson had recommended painting the abdomen with iodine. The medical man who had attended the lady had gone direct from a scarlet fever patient. He had himself delivered a woman just a few days ago, in a house where there was scarlet fever. He thought every precaution should be taken, such as washing the hands, etc.; and, if possible, medical men should see their puerperal patients before visiting infectious cases.

Dr Jefferiss said that he had followed this practice for very many years, and he had never lost a patient. He had often seen great mischief done by students attending midwifery cases while treating infectious diseases.

Dr Bell thought it was as dangerous to attend confinement cases after treating erysipelas, as after scarlet fever.

Dr Gordon did not think there was very much danger if the ordinary precautions were observed.

Dr Young said that, as a rule, he refused to attend women in confinement if he had puerperal fever in his practice; but it so happened, that lately he was forced to go directly from a fever case to another labour, but mischief resulted.

CASE OF RUPTURED OVARY.

By L. R. Thomson, M.D., Dalkeith.

July 26, 1871.

Dr Thomson verbally narrated the particulars of a recent case in which rupture of the ovary had occurred.

Dr Menzies remembered a case in which Dr Keith removed a large ovarian tumour. The other ovary looked suspicious, and he was just about to remove it also, but on second thought it was allowed to remain. Since then the patient had given birth to a child.

Professor Simpson said, that hæmatoceles were frequently caused by rupture of the ovary. Rupture of ovary does not always prove fatal.

Dr Sidey said that the last ovarian patient he had, had been operated on by Dr Keith. She subsequently had a child, and then disease coming on in the remaining ovary, she was operated on again and died.

Dr Ballantyne suggested that the symptoms in Dr Thomson's case might have been caused by the tapping, and not by rupture at all.

A FEW NOTES ON A CASE OF PHLEGMONOUS INFLAMMATION OF THE LEFT LABIUM, AND ABSCESS.

By A. M. T. Rattray, M.D., Portobello.

July 26, 1871.

The diseases which affect the labia are often the source of great annoyance and uneasiness to patients, many of whom, through false or misplaced modesty, suffer rather than solicit the advice of the accoucheur.

In this short paper I will only mention a few of the affections commonly met with in this region. 1. Hypertrophy of the labia is seldom seen. 2. Varicose veins of the labia. The late Sir James Simpson, at page 305, vol. i., of his Obstetric Works, mentions a case of fatal venous hæmorrhage from the pudendi (*i.e.*, the labia externa). Dr J. Kyle, of Dundee, reported the case to him, and in bringing it before the Fellows of the Obstetrical Society in 1849, Sir James alluded to the anatomical fact that there "was at the foot of each labium a plexus of veins (large), which extended some way into the vagina, one of these veins, probably in a varicose state,

had burst in this instance." In all probability, besides dilatation, there was thickening of the coat of the vein, and therefore it would not collapse, as veins usually do, but remained patent, like an elastic artery."

3. Oozing tumour of labia is chiefly characterised by heat, irritation, enlargement, and an aqueous-acrid discharge. Sir C. M. Clarke thinks that to an uninitiated eye it might be mistaken for that form of erysipelas called shingles.

4. Fibrous tumours I shall pass over, merely referring to an interesting case, which is well described at page 378, vol. xxv., of "Braithwaite's Retrospect of Medicine."

Another growth with which phlegmon of the labium might, on a superficial examination, be confounded, is (5.) The Encysted. It is soft and embedded in one or both labia, and over it the skin moves freely, no tenderness on pressure is felt, and its growth is slow, whereas phlegmonous inflammation usually runs a rapid course, pain, heat, redness, swelling and throbbing, quickly follow each other, and make the sufferer miserable, and whatever be the position in which she may be placed, pain is acute, especially in walking, as can well be imagined from the situation of the parts involved.

This disease, according to Dr Churchill, consists ordinarily of "inflammation of the skin and subcutaneous tissue, and may attack females at any age, and occupy one or both labia." Dr Milne, in his recent work on the "Principles and Practice of Midwifery," at page 22, thus describes the labia,—"The labia externa, or majora, or pudendi, are two duplications or folds; skin externally, and mucous membrane internally, extending downwards from the sides of the mons veneris to the fourchette;" and Dr Ramsbotham designates them "pouting lips," about three inches in length.

M. Hugnier has demonstrated that in most cases "the abscesses of the labium have their original seat in the vulvo-vaginal gland; either the duct is obstructed, and the accumulation of secretion sets up inflammation, or

inflammation spreads along the duct to the gland." With this view Dr Churchill inclines to coincide. For a full description of the external organs of generation, see his (Dr C.'s) work on the "Diseases of Women," 5th edition, and page 30.

On June 10th, 1871, I was asked to see Mrs B. She is thin, short, and of lymphatic temperament. Her age is 28. She tells me her mother died in 1862, of bronchitis, and her father of heart disease in 1854. Has one sister living, who is not very robust.

Mrs B. was first confined on September 13th, 1868. Two months previous to her confinement, she was seized one morning with rigors, and began to suffer uneasiness and discomfort in walking or sitting, whereupon her attention was directed to a swelling in the left side of the vagina; it was painful to the touch, and compelled her to seek retirement, in consequence of the constant and distressing itching, and desire to scratch the part. She then consulted two medical men on separate occasions, the one advised non-interference, the other incision. The swelling did partly disappear before her accouchement, but what fulness remained did not retard labour, which was quick and normal. Menstruation has always been regular, even to a day. She is again pregnant, and last menstruated on February 4, 1871.

To my no small surprise, a swelling, preceded by chills, and much similar to that with which she was affected in July 1868, manifested itself on 22d of April. It was the size of a large hen's egg, and harder and painful to the touch, and had steadily increased till it attained the size of a goose egg at the lower part. On June 10th, as stated above, I saw the case. She complained of great pain, which extended all over the thigh, through the pelvis, and midway up the spine. The abscess gave the feeling of fluctuation. It was red, congested, and swollen, and the mere application of the finger caused the patient to show unmistakable signs of pain. Perfect rest and quiet were at once enjoined; and, to allay irritability, and

to gently act upon the bowels, I prescribed Extracti cannabis Indicæ gr. ii., Mas. pil. hydrargi., Extracti hyoscyami, Mas. pil. colocynth comp. aa. gr. viii., M. et divid. in pilulas æquales octo. Signat. One pill occasionally as a laxative.

June 11*th*.—Slept well, though occasionally annoyed with pricking and throbbing pains in the swelling. The bowels were freely operated upon by the pill of the previous day. Pulse 84, firm. Tongue clean. Mrs B. informs me that, on repeated occasions, she experienced considerable difficulty in passing water.

13*th*.—To-day, at visit, having made certain of the presence of matter, and to avoid any risk of a fistulous opening of the abscess, should it be left to nature, I laid it freely open; a great gush of dark brown fetid pus, largely streaked with blood, followed the operation. Boivin and Dugès relate a case in which the matter burrowed and formed a fistulous opening into the rectum. Linseed meal and charcoal poultices, made with decoction of poppy heads, were for a few days constantly applied to the abscess, and the patient was kept in the supine position.

14*th*.—Had a restless night. The abscess is almost as large as ever. It feels soft and boggy. I opened it, and large dark clotted masses of venous-like blood were evacuated, having a most nauseating odour.

15*th*.—Greatly improved to-day. Tongue clean and moist; pulse 76. Slept well all night. Marked reduction in the size, the swelling and pain are almost completely gone; no matter appears now. Mercurial ointment to be applied over the parts twice daily.

16*th*.—Irritation of the bladder, and difficulty in micturating, interfere with her night's repose. Ordered ʒss. castor oil immediately, and in three or four hours, ʒss. dose of compound jalap powder.

17*th*.—Has had an excellent night, which she attributed to the medicine of the previous day; the bowels were thoroughly moved, and the flow of urine was very copious.

19*th*.—The mercurial ointment has so reduced the swelling, that there exists now scarcely a trace of it.

21*st*.—To-day she has been out of bed, and is able, with some measure of ease and comfort, to attend to her usual domestic duties. Her only complaint now is weakness, for which she got a mixture containing Quin. et citrat. ferri ʒi., Tinct. aurantii ʒss., Infus. calumbæ ʒvii. Sig. ʒss. Ante cibum, in aqua.

I was again sent for, on 16th July, when she took suddenly ill with a violent and spasmodic attack of breathlessness and palpitation, etc. She could only account for such an attack by taking too long a walk on the previous Saturday evening. I prescribed Bromid. potass. ʒii., Hydrat. chloral. ʒiii., Tinct. aurantii ʒss., Tinct. ammon. valerian ʒvi. Aq. cinnam. ʒvii.—ʒss. quarta quaque hora, in aqua. In a few days all these disagreeable symptoms vanished; and when last seen by me, on the 23d inst., she pronounced herself in better health than on any former occasion. She was happy and cheerful, and rejoicing in the perfect freedom from pain, which so long rendered her life a burden, and caused constant and increasing depression of spirits.

MEMORANDA ON CARBONATE OF LITHIA IN ACUTE RHEUMATISM ASSOCIATED WITH SCARLATINA, AND IN URTICARIA; ETC.

BY JNO. BURTON, M.D., WALSALL.

JULY 26, 1871.

HAVING lately had no less than seven cases of acute rheumatism in nine cases of scarlatina, in infants from 3 to 7 years of age, I administered 3 to 5 grains of carbonate of lithia, of *German make*, in milk three times a-day with very marked success. I have not found this preparation made in England so good as that made at Bonn.

In urticaria, also in doses of 15 grs., three or four times a-day, if the stomach will bear so much, administered to adults suffering from acute febrile urticaria, I have had a most gratifying success.

A little nausea is not to be heeded; but in one case positive vomiting caused me to diminish the dose, and finally to substitute the citrate of lithia, with very palpable diminution of the curative effects.

Butter Milk as Food for Infants.—I would refer to Vol. I. *Medical Times and Gazette*, No. 1029, March 19, 1870, page 305, and No. 1030, March 26, page 330, for a paper on this subject, by A. M. Ballot, M.D., of Rotterdam, for somewhat full details on this matter, and will only state that in ten cases in my practice, since that paper was published, the results have fully borne out the statements and experience of Dr Ballot. In the first we did not attend very particularly to the *weights* and *measures* of Dr Ballot's formula, but merely boiled the butter milk for a minute, adding a little *fine white "biscuit flour,"* so as not to make it too thick for sucking, and adding sugar to bring the mixture to a desirable degree of sweetness; finding that, although the vomiting, diarrhœa, screaming, and want of sleep were immediately abated, and the child much improved in appearance, that it did not get *fat*, I added to each bottle of food about ʒss. of fine sweet almond oil; and further, in consequence of flatulence, about an equal quantity of good French brandy. The result was perfect. The child digested perfectly, and soon became plump and healthy, instead of being the wretched cadaverous skeleton it was when the treatment began; and it is now about fourteen months old, in the midst of teething, a fine healthy infant,—in a family where several of its predecessors have died from artificial feeding,—*the parents being unhealthy.* In the second case, the good effects of this feeding were equally prompt and striking; but here we had to make our own butter milk, by taking the new milk and setting it in a warm place to get sour, and churning it in a bottle, straining off the butter through

a muslin,—a "*dodge*" worth notice in towns and districts where butter milk cannot be easily had *good* or fresh.

N.B.—The butter milk in the first case was churned twice a-week, and used as it was required, so that it was often three days old at the time it was used.

CASE OF CONVULSIONS OCCURRING AFTER DELIVERY.

By ALEXANDER KEILLER, M.D.

JULY 26, 1871.

ANNE M'PHERSON, æt. 20, was admitted to the Maternity Hospital on Tuesday, 20th June 1871, and was delivered of a healthy male child at 4.20 P.M. of the same day.

She continued perfectly well until 6 A.M. on Friday, the 23d, when she complained to the nurse that she could not see. About ten minutes afterwards she had a fit of an epileptiform character. It was not of very long duration, but a similar fit occurred after the lapse of about half an hour, and was succeeded by others, with increasing frequency. About mid-day she was seen by Dr Keiller, who ordered her to have acetate of potash in pretty large doses, and chloroform if the fits were violent. The urine was examined at this time, and found to contain half of albumen. The pulse was 160, hard and incompressible, except during the fits, when it became quite imperceptible, and the heart's action ceased almost entirely. The temperature was 104°.5. After Dr Keiller's visit, the fits increased in both frequency and violence, and chloroform producing no effect upon them, the experiment was tried of putting her under chloroform during the intervals. This was done with complete success. As long as she was kept fully under chloroform, the fits were cut short, and consisted of nothing more than a slight twitching of the muscles of the arms and face. If the chloroform was suspended for a minute she had a fit of the original

violence. This continued till between 7 and 8 o'clock in evening, when the fits ceased. She continued however to toss her arms and head about, and moan. About half past 8 she was again seen by Dr Keiller, who ordered ice to the head, and hot mustard cloths to be wrapped round the legs, also 10 grains of calomel to be laid on the tongue.

During the course of this day (Friday), she had, in all, twenty-nine complete fits, and eighteen abortive ones (under chloroform).

Saturday, 24th.—Patient vomited a little in the morning, but remained in a semicomatose state all day, giving no sign of consciousness. Her bowels were moved three times during the night.

Sunday, 25th.—In the forenoon, patient showed some signs of consciousness. She took a little beef-tea, and some of the acetate of potash, which it had been impossible to get her to take before. She was seen by Dr Bell, who ordered her hair to be thinned, and mustard to be applied to the back of the neck.

Monday, 26th.—Patient very much better physically, and taking her food pretty well. Signs of mental disturbance beginning to shew themselves pretty markedly (she had been talking a little nonsense the previous evening). She maintains that the afterbirth hasn't been removed, and begs piteously for its removal.

Tuesday, 27th.—Mental aberration continues. She has hallucinations about men looking in at her window (two storeys high). She is very much afraid that she is going to die, thinks that men are outside making her coffin, that the nurses are working at her winding-sheet, etc., etc. The urine still contains a considerable quantity of albumen. Her pulse is not much over the normal standard, and her appetite is good. A copious eruption of herpes labialis has appeared round her mouth.

July 5th.—Patient was dismissed to-day quite well. About a couple of days after the date of the last notice she became quite rational, and her recovery afterwards was rapid, without a bad symptom.

FATTY PLACENTA.

EXHIBITED BY JAMES YOUNG, M.D.

JULY 26, 1871.

DR YOUNG showed a placenta from a patient who was confined on Saturday last. It was her fourth confinement. The first child only lived twenty-four hours, and had symptoms of morbus cæruleus; the second lived ten days; the third was still-born, and the last child also showed some signs of cyanosis. Sir James Simpson had seen the patient, and recommended the use of chlorate of potass. During each pregnancy, patient complained of pain in right side of the abdomen. The placenta, he thought, presented some of the characters of fatty degeneration.

Dr Keiller said that in such cases there was usually something connected with the condition of the general health which accounted for the impairment in the child's condition. Syphilis, for instance, was a common cause of premature births, but somtimes the effects of the poison wear out, and living children are born.

Professor Simpson thought the spots on the placenta were of the nature of hæmorrhagic deposits.

Dr Cairns said he had seen the placenta loaded with fat, and the child nevertheless healthy; whereas, in other cases, the afterbirth was apparently free from disease, and the child was still-born. He had never found much benefit from the chlorate of potass. He mentioned a case in which much good resulted from a mercurial course to both father and mother, and he was of opinion that blue pill was more efficacious than chlorate of potass.

Dr Cochrane was of opinion that, in Dr Young's patient, the diseased condition of the children was most probably due to syphilis. He did not believe in chlorate of potass in such cases, and thought that much good might often be done by the judicious administration of mercury.

Dr Menzies mentioned the case of a woman who had lost two children, evidently from syphilis. He gave the chlorate of potass, and a fine healthy child was born at the full time. In another case, the mother had lost six children, and after a course of the same remedy, a healthy child was born.

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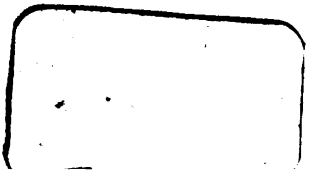
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